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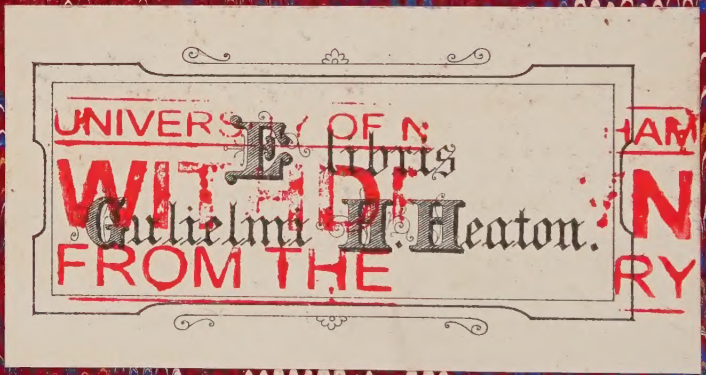

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CORRELATION

OF

PHYSICAL FORCES

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William A. Heaton.

THE CORRELATION OF PHYSICAL FORCES.

SIXTH EDITION.

WITH OTHER CONTRIBUTIONS TO SCIENCE.

BY THE HON.

SIR W. R. GROVE, M.A., F.R.S.

ONE OF THE JUDGES OF THE COURT OF COMMON PLEAS.

LONDON:
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PREFACE

TO

THE SIXTH EDITION.

The fifth edition of the 'Correlation of Physical Forces' having been exhausted, I have done my best during the few spare hours at my disposition to prepare a sixth. Partly induced by the suggestions of others, partly, perhaps, by egotism, I have added a selection of papers on scientific subjects, which I have from time to time previously published. Physical science has moved on far too quickly for me to follow it, and no one is more convinced than I am that

> To have done, is to hang
> Quite out of fashion, like a rusty mail,
> In monumental mockery.

Still, having gone through my past contributions to science, forgotten by all others, half-forgotten by myself, I find something of suggestion in most of them which may not be altogether useless ; and as the papers are scattered about in various periodicals, a collection of them in one volume may be accepted as a last legacy.

I have read all with care. I have added a few paragraphs here and there to the Essays; the other papers I have not altered, farther than by correcting some mere verbal errors, and, in two or three instances, incorporating with the text, paragraphs printed at the time as footnotes.

I have prefixed the dates and names of the volumes in which they were published; and though in their present arrangement I have not adhered strictly to chronological order, I have done so as far as is consistent with papers on the same subjects following each other.

I have entitled them 'Experimental Investigations,' to avoid confusion with the better title, appropriated by Faraday, of 'Experimental Researches.'

Towards the end I have given a few observations of little value in themselves, but made on phenomena of rare occurrence. While this edition was going through the press Sir C. Wheatstone has called my attention to a book entitled 'Élemens de Physique Générale, par Jules Guyot, Paris, 1832,' in which, though the style of argument is *à priori* and somewhat dogmatic, the author attributes natural phenomena to motion either of translation, as he terms it, or molecular, and attacks the theories of fluids, ethers, &c. It is a work of considerable merit, and shows, when conjoined with the references I have given, that many minds are independently converging to the same theory, the which I believe will ultimately prevail.

PREFACE

TO

THE FIFTH EDITION.

The phrase 'Correlation of Physical Forces,' in the sense in which I have used it, having become recognised by a large number of scientific writers, it would produce confusion were I now to adopt another title. It would, perhaps, have been better if I had in the first instance used the term Co-relation, as the words 'correlate,' 'correlative,' had acquired a peculiar metaphysical sense somewhat differing from that which I attached to the substantive correlation. The passage in the text (p. 165) explains the meaning I have given to the term.

Twenty-five years having elapsed since I promulgated the views contained in this Essay, which were first advanced in a lecture at the London Institution, in January 1842, printed by the Proprietors, and subsequently more fully developed in a course of lectures in 1843, published in abstract in the 'Literary Gazette,' I think it advisable to add a little to the Preface with reference to other labourers in the same field.

It has happened with this subject, as with many

others, that similar ideas have independently presented themselves to different minds about the same period. In 'Liebig and Wohler's Annalen' for May 1842,* a paper appeared by M. Mayer, which I had not read when my third edition was published, but which I have now read in the translation by Mr. Youmans, of New York. It deduces very much the same conclusions to which I had been led, the author starting partly from *à priori* reasoning and partly from an experiment by which water was heated by agitation; and from another, which had, however, previously been made by Davy, viz. that ice can be melted by friction, though kept in a medium which is below the freezing-point of water.

In 1843 a paper by Mr. Joule on the mechanical equivalent of heat appeared, which, though not in terms touching on the mutual and necessary dependence of all the Physical Forces, yet bears most importantly upon the doctrine.

While my third edition was going through the press I had the good fortune to make the acquaintance of M. Seguin, who informed me that his uncle, the eminent Montgolfier, had long entertained the idea that force was indestructible, though, with the exception of one sentence in his paper on the hydraulic ram, and where he is apparently speaking of mechanical force, he has left nothing in print on the subject. Not so, however, M. Seguin himself, who in 1839, in a work on the 'Influence of Railroads,' has distinctly expressed

* I am informed that these papers are, in fact, published some time after the date which they bear in the magazine.

his uncle's and his own views on the identity of heat and mechanical force, and has given a calculation of their equivalent relation, which is not far from the more recent numerical results of Mayer, Joule, and others.

Several of the great mathematicians of a much earlier period advocated the idea of what they termed the Conservation of Force; but although they considered that a body in motion would so continue for ever, unless arrested by the impact of another body—and, indeed, in the latter case, would, if elastic, still continue to move (though deflected from its course) with a force proportionate to its elasticity—yet with inelastic bodies the general and, as far as I am aware, the universal belief was, that the motion was arrested on impact and the force annihilated. Montgolfier went a step farther, and his hydraulic ram was to him a proof of the truth of his preconceived idea, that the shock or impact of bodies left the mechanical force undestroyed.*

Previously, however, to the discoveries of the voltaic battery, electro-magnetism, thermo-electricity, and photography, it was impossible for any mind to perceive what, in the greater number of cases, became of the force which was apparently lost. The phenomena of heat, known from the earliest times, would have been a mode of accounting for the resulting force in many cases where motion was arrested, and we find Bacon enouncing a theory that motion was the form, as he quaintly termed it, of heat. Rumford and Davy

* See also a paper by Mr. Rankine, *On the Dynamical Principles of Newton*, in the *Engineer*, Oct. 26, 1866.

adopted this view, the former with a fair approximative attempt at numerical calculation, but no one of these philosophers seems to have connected it with the indestructibility of force. A passage in the writings of Dr. Roget, combating the theory that mere contact of dissimilar bodies was the source of voltaic electricity, philosophically supports his argument by the idea of non-creation of force.

As I have introduced into the later editions of my Essay abstracts of the different discoveries which I have found, since my first lectures, to bear upon the subject, I have been regarded by many rather as the historian of the progress made in this branch of thought than as one who has had anything to do with its initiation. Every one is but a poor judge where he is himself interested, and I therefore write with diffidence; but it would be affecting an indifference which I do not feel if I did not state that I believe myself to have been the first who introduced this subject as a generalised system of philosophy, and continued to enforce it in my lectures and writings for many years, during which it met with the opposition usual and proper to novel ideas.

Avocations necessary to the well-being of others have prevented my following it up experimentally, to the extent that I once hoped; but I trust and believe that this Essay, imperfect though it be, has helped materially to impress on that portion of the public which devotes its attention rather to the philosophy of science than to what is now termed science, the truth of the thesis advocated.

To show that the work of to-day is not substantially different from the thoughts I first published on the subject, at a period when I knew little or nothing of what had been thought before, I venture to give a few extracts from the printed copy of my lecture of 1842:—

Physical Science treats of Matter, and what I shall to-night term its *Affections*; namely, Attraction, Motion, Heat, Light, Electricity, Magnetism, Chemical-Affinity. When these re-act upon matter, they constitute Forces. The present tendency of theory seems to lead to the opinion that all these Affections are resolvable into one, namely, Motion: however, should the theories on these subjects be ultimately so effectually generalised as to become laws, they cannot avoid the necessity for retaining different names for these different Affections; or, as they would then be called, different Modes of Motion. . . . .

Œrsted proved that Electricity and Magnetism are two forces which act upon each other; not in straight lines, as all other known forces do, but in a rectangular direction: that is, that bodies invested with electricity, or the conduits of an electric-current, tend to place magnets at right angles to them; and, conversely, that magnets tend to place bodies conducting electricity at right angles to them. . . . .

The discovery of Œrsted, by which electricity was made a source of Magnetism, soon led philosophers to seek the converse effect; that is, to educe Electricity from a permanent magnet. Had these experimentalists succeeded in their expectations of making a stationary magnet a source of electric-currents, they would have realised the ancient dreams of perpetual motion, they would have converted statics into dynamics, they would have produced power without expenditure; in other words, they would have become creators. They failed, and Faraday saw their error: he proved that to

obtain Electricity from Magnetism it was necessary to superadd to this latter, motion; that magnets while in motion induced electricity in contiguous conductors; and that the direction of such electric-currents was tangential to the polar direction of the magnet; that as Dynamic-electricity may be made the source of Magnetism and Motion, so Magnetism conjoined with Motion may be made the source of Electricity. Here originates the Science of Magneto-electricity, the true converse of Electro-magnetism; and thus between Electricity and Magnetism is shown to exist a reciprocity of force such that, considering either as the primary agent, the other becomes the re-agent; viewing one in the relation of cause, the other is the effect. . . . .

The Science of Thermo-electricity connected heat with electricity, and proved these, like all other natural forces, to be capable of mutual reaction. . . . .

Voltaic action is Chemical action taking place at a distance, or transferred through a chain of media; and the Daltonian equivalent numbers are the exponents of the amount of voltaic action for corresponding chemical substances. . . . .

By regarding the quantity of electrical, as directly proportional to the efficient chemical action, and by experimentally tracing this principle, I have been fortunate enough to increase the power of the Voltaic-pile more than sixteen times, as compared with any combination previously known. . . . .

I am strongly disposed to consider that the facts of Catalysis depend upon voltaic action, to generate which three heterogeneous substances are always necessary. Induced by this belief, I made some experiments on the subject, and succeeded in forming a voltaic combination by gaseous-oxygen, gaseous-hydrogen, and platinum, by which a galvanometer was deflected and water decomposed. . . . .

It appears to me that heat and light may be considered as affections; or, according to the Undulatory theory, vibrations of matter itself, and not of a distinct ethereal fluid permeating it. These vibrations would be propagated, just as sound is propagated by vibrations of wood or as waves by water. To my mind, all the consequences of the Undulatory theory flow as easily from this as from the hypothesis of a specific ether; to suppose which—namely, to suppose a fluid *sui generis*, and of extreme tenuity, penetrating solid bodies—we must assume, first, the existence of the fluid itself; secondly, that bodies are, without exception, porous; thirdly, that these pores communicate; fourthly, that matter is limited in expansibility. None of these difficulties apply to the modification of this theory which I venture to propose; and no other difficulty applies to it which does not equally apply to the received hypothesis. With regard to the planetary spaces, the diminishing periods of comets is a strong argument for the existence of an universally-diffused matter: this has the function of resistance, and there appears to be no reason to divest it of the functions common to all matter, or specifically to appropriate it to certain affections. Again, the phenomena of transparency and opacity are, to my mind, more easily explicable by the former than by the latter theory, as resulting from a difference in the molecular arrangement of the matter affected. In regard to the effects of double-refraction and polarisation, the molecular structure gives at once a reason for the effects upon the one theory, while upon the other we must, in addition to previous assumptions, farther assume a different elasticity of the ether in different directions within the doubly-refracting medium. The same theory is applicable to Electricity and Magnetism; my own experiments on the influence of the elastic intermedium on the voltaic-arc, and those of Faraday on electrical induction, furnish strong arguments in support of it. My

inclination would lead me to detain you on this subject much longer than my judgment deems advisable; I therefore content myself with offering it to your consideration, and, should my avocations permit, I may at a future period more fully develop it. . . . .

Light, Heat, Electricity, Magnetism, Motion, and Chemical-affinity, are all convertible material affections; assuming either as the cause, one of the others will be the effect: thus heat may be said to produce electricity, electricity to produce heat; magnetism to produce electricity, electricity magnetism; and so of the rest. Cause and effect, therefore, in their abstract relation to these forces, are words solely of convenience. We are totally unacquainted with the ultimate generating power of each and all of them, and probably shall ever remain so; we can only ascertain the normæ of their action: we must humbly refer their causation to one omnipresent influence, and content ourselves with studying their effects and developing, by experiment, their mutual relations.

I have transposed the passages relating to voltaic action and catalysis, but I have not added a word to the above quotations; and, as far as I am now aware, the theory that the so-called imponderables are affections of ordinary matter, that they are resolvable into motion, that they are to be regarded in their action on matter as forces, and not as specific entities, and that they are capable of mutual reaction, thence alternately acting as cause and effect, had not at that time been publicly advanced.*

My original Essay being a record of lectures, and being published by the Managers of the Institution, I

* See also an experiment shown at the London Institution, p. 196, post.

necessarily adhered to the form and matter which I had orally communicated. In preparing subsequent editions I found that, without destroying the identity of the work, I could not alter the style; although it would have been less difficult and more satisfactory to me to have done so, the work would not then have been a republication; and I was for obvious reasons anxious to preserve as far as I could the original text, which, though added to, is but little altered.

The form of lectures has necessarily continued the use of the first person, and I would beg my readers not to attribute to me, from the modes of expression used, a dogmatism which is far from my thought. If my opinions are expressed broadly, the reason is that when opinions are always hedged in by qualifications, the style becomes embarrassed and the meaning frequently unintelligible.

As the main object of a course of lectures is to induce the auditor to think, and to consult works on the subject he hears treated, so the object of this Essay is more to induce a particular train of thought on the known facts of physical science than to enter with minute criticism into each separate branch.

In one or two of the reviews of previous editions the general idea of the work was objected to. I believe, however, that will not now be the case; the mathematical labours of Sir W. Thompson, Clausius, and others, though not suitable for insertion in an Essay such as this, have awakened an interest for many portions of the subject, which promises much for its future progress.

The short and irregular intervals which my profes-

sion permits me to devote to science so prevent the continuity of attention necessary for the proper evolution of a train of thought, that I certainly should not now have courage to publish for the first time such an Essay; and it is only the favour it has received from those whose opinions I highly value, and the, I trust pardonable, wish not to let some favourite thoughts of my youth lose all connection with my name, that have induced me to reprint it.

My scientific readers will, I hope, excuse the very short notices of certain branches of science which are introduced, as without them the work would be unintelligible to many for whom it is intended. I have endeavoured so to arrange the subjects that each division should form an introduction to those which follow, and to assume no more preliminary knowledge to be possessed by my readers than would be expected from persons acquainted with the elements of physical science.

The notes contain references to the original memoirs in which the branches of science alluded to are to be found, as well as to those which bear on the main arguments; where these memoirs are numerous, or not easy of access, I have referred to treatises in which they are collated. To prevent the reader's attention being interrupted, I have in the notes referred to the pages of the text, instead of to interpolated letters.

For this fifth edition I have revised the whole of the text; and several correspondents having suggested that I should add to it my address as President of the British Association, 1866, it seemed to me on consi-

deration not inappropriate. In it are noticed several new discoveries relating to the subjects treated of in this Essay, which, had I not decided on adding the discourse, I should have incorporated with the text. The doctrine of Continuity, however, though closely allied with that of Correlation, could not well have been interwoven with the Essay, and is more suitable as a sequel.

I cannot but feel gratified with the reception that address has met with; and although the portions referring to the continuity of succession of organised beings have, as I rather expected, been the subject of some adverse comment, yet, as I have seen no argument against them, I have nothing at present to answer or to alter.

Several of the arguments adduced by me were written some years ago, and before the appearance of Darwin's now celebrated work. I had then no notion of the effects of natural selection in modifying organisms, but in order to test as fairly as I could the reasons for and against continuity, as opposed to special creations, I wrote down at different times in the form of a dialogue everything that occurred to me as bearing most strongly on each side of the question, and showed it to several friends with whom I discussed the subject. My resulting opinion is given in the text.

I thought I had sufficiently guarded myself in the passage at p. 221, from being supposed to deny that there are or have been catastrophes or cataclysms; but it appears from some comments that I have not done so. If sea or river undermine a cliff and the cliff fall,

it is undoubtedly a cataclysm; if I tread on a beetle, it is a catastrophe to the beetle; but formation and destruction are very different things, and the tenour of my discourse applies to genesis, not extinction. Even in phenomena such as those of Geology, though there were doubtless cataclysms, and sometimes on a much larger scale than at other times, yet the evidence seems to me to point to their being limited in extent at any one period, when compared with the whole terrestrial surface.

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CORRELATION

OF

PHYSICAL FORCES.

WHEN natural phenomena are for the first time observed, a tendency immediately developes itself to refer them to something previously known—to bring them within the range of acknowledged sequences. The mode of regarding new facts, which is most favourably received by the public, is that which refers them to recognised views—stamps them into the mould in which the mind has been already shaped. The new fact may be far removed from those to which it is referred, and may belong to a different order of analogies, but this cannot then be known, as its co-ordinates are wanting. It may be questionable whether the mind is not so moulded by past events that it is impossible to advance an entirely new view; but, admitting such possibility, the new view, necessarily founded on insufficient data, is likely to be more incorrect and prejudicial than even a strained attempt to reconcile the new discovery with known facts.

The theory consequent upon new facts, whether it be a co-ordination of them with known ones, or the more difficult and dangerous attempt at remodelling the public ideas, is generally enunciated by the discoverers themselves of the facts, or by those to whose authority the world at the period of the discovery defers; others are not bold enough, or if they be so, are unheeded. The earliest theories thus enunciated obtain the firmest hold upon the public mind, for at

such a time there is no power of testing, by a sufficient range of experience, the truth of the theory; it is accepted solely or mainly upon authority: there being no means of contradiction, its reception is, in the first instance, attended with some degree of doubt, but as the time in which it can fairly be investigated far exceeds that of any lives then in being, and as neither the individual nor the public mind will long tolerate a state of abeyance, a theory shortly becomes, for want of a better, admitted as an established truth: it is handed from father to son, and gradually takes its place in education. Succeeding generations, whose minds are thus formed to an established view, are much less likely to abandon it. They have adopted it, in the first instance, upon authority to them unquestionable, and subsequently to yield up their faith would involve a laborious remodelling of ideas, a task which the public as a body will and can rarely undertake, the frequent occurrence of which is indeed inconsistent with the very existence of man in a social state, as it would induce an anarchy of thought—a perpetuity of mental revolutions.

This necessity has its good; but the prejudicial effect upon the advance of science is, that by this means theories the most immature frequently become the most permanent; for no theory can be more immature, none is likely to be so incorrect, as that which is formed at the first flush of a new discovery; and though time exalts the authority of those from whom it emanated, time can never give to the illustrious dead the means of analysing and correcting erroneous views which subsequent discoveries confer.

Take, for instance, the Ptolemaic System, which we may almost literally explain by the expression of Shakspeare: 'He that is giddy thinks the world turns round.' We now see the error of this system, because we have all an immediate opportunity of refuting it; but this identical error was received as a truth for centuries, because, when first promulgated, the means of refuting it were not at hand; and when the means of its refutation became attainable, mankind had been so educated to the supposed truth, that they rejected the proof of its fallacy.

I have premised the above for two reasons: first, to obtain a fair hearing, by requesting as far as possible a dismissal from the minds of my readers of preconceived views by and in favour of which all are liable to be prejudiced; and, secondly, to defend myself from the charge of undervaluing authority, or treating lightly the opinions of those to whom and to whose memory mankind looks with reverence. Properly to value authority we should estimate it together with its means of information: if 'a dwarf on the shoulders of a giant can see farther than the giant,' he is no less a dwarf in comparison with the giant.

The subject on which I am about to treat—viz. the relation of the affections of matter to each other and to matter—peculiarly demands an unprejudiced regard. The different aspects under which these agencies have been contemplated; the different views which have been taken of matter itself; the metaphysical subtleties to which these views unavoidably lead, if pursued beyond fair inductions from existing experience, present difficulties almost insurmountable.

The extent of claim which my views on this subject may have to originality has been stated in the Preface; they became strongly impressed upon my mind at a period when I was much engaged in experimental research, and were, as I then believed, and still believe, regarding them as a system, new: expressions in the works of different authors, bearing more or less on the subject, have subsequently been pointed out to me, some of which go back to a distant period. An attempt to analyse these in detail, and to trace how far I have been anticipated by others, would probably but little interest the reader, and in the course of it I should constantly have to make distinctions showing wherein I differed, and wherein I agreed with others. I might cite authorities which appear to me to oppose, and others which appear to coincide, with certain of the views I have put forth; but this would interrupt the consecutive development of my own ideas, and might render me liable to the charge of misconstruing those of others; I therefore think it better to avoid such discussion in the text; and in addition to the sketch given in the Preface,

to furnish in the notes at the conclusion such references to different authors as bear upon the subjects treated of, which I have discovered, or which have been pointed out to me since the delivery of the lectures of which this essay is a record.

The more extended our research becomes, the more we find that knowledge is a thing of slow progression, that the very notions which appear to ourselves new, have arisen, though perhaps in a very indirect manner, from successive modifications of traditional opinions. Each word we utter, each thought we think, has in it the vestiges, is in itself the impress, of antecedent words and thoughts. As each material form, could we rightly read it, is a book, containing in itself the past history of the world ; so, different though our philosophy may now appear to be from that of our progenitors, it is but theirs added to or subtracted from, transmitted drop by drop through the filter of antecedent, as ours will be through that of subsequent, ages. The relic is to the past as is the germ to the future.

Though many valuable facts, and correct deductions from them, are to be found scattered amongst the voluminous works of the ancient philosophers ; yet, giving them the credit which they pre-eminently deserve for having devoted their lives to purely intellectual pursuits, and for having thought, seldom frivolously, often profoundly, nothing can be more difficult than to seize and apprehend the ideas of those who reasoned from abstraction to abstraction—who, although, as we now believe, they must have depended upon observation for their first inductions, afterwards raised upon them such a complex superstructure of syllogistic deductions, that, without following the same paths, and tracing the same sinuosities which led them to their conclusions, such conclusions are to us unintelligible. To think as another thought, we must be placed in the same situation as he was placed : the errors of commentators generally arise from their reasoning upon the arguments of their text, either in blind obedience to its dicta, without considering the circumstances under which they were uttered, or in viewing the images presented to the original writer from a different point to that from which he viewed them. Experimental philosophy keeps in check

the errors both of *à priori* reasoning and of commentators, and, at all events, prevents their becoming cumulative; though the theories or explanations of a fact be different, the fact remains the same. It is, moreover, itself the exponent of its discoverer's thought: the observation of known phenomena has led him to elicit from nature the new phenomenon: and though he may be wrong in his deductions from this after its discovery, the reasonings which conducted him to it are themselves valuable, and having led from known to unknown truths, can seldom be uninstructive.

Very different views existed amongst the ancients as to the aims to be pursued by physical investigation, and as to the objects likely to be attained by it. I do not here mean the moral objects, such as the attainment of the *summum bonum*, &c., but the acquisitions in knowledge which such investigations were likely to confer. Utility was one object in view, and this was to some extent attained by the progress made in astronomy and mechanics. Archimedes, for instance, seems to have constantly had this end in view; but, while pursuing natural knowledge for the sake of knowledge, and the power which it brings with it, the greater number seemed to entertain an expectation of arriving at some ultimate goal, some point of knowledge, which would give them a mastery over the mysteries of nature, and would enable them to ascertain what was the most intimate structure of matter, and the causes of the changes it exhibits. Where they could not discover, they speculated. Leucippus, Democritus, and others have given us their notions of the ultimate atoms of which matter was formed, and of the *modus agendi* of nature in the various transformations which matter undergoes.

The expectation of arriving at ultimate causes or essences continued long after the speculations of the ancients had been abandoned, and continues even to the present day to be a very general notion of the objects to be ultimately attained by physical science. Francis Bacon, the great remodeller of science, entertained this notion, and thought that, by experimentally testing natural phenomena, we should be enabled to trace them to certain primary essences or causes whence the various phenomena flow. These he speaks of under the

scholastic name of 'forms'—a term derived from the ancient philosophy, but differently applied. He appears to have understood by 'form' the essence of quality—that in which, abstracting everything extraneous, a given quality consists, or that which, superinduced on any body, would give it its peculiar quality: thus, the form of transparency is that which constitutes transparency, or that by which, when discovered, transparency could be produced or superinduced. To take a specific example of what I may term the synthetic application of his philosophy:—'In gold there meet together yellowness, gravity, malleability, fixedness in the fire, a determinate way of solution, which are the simple natures in gold; for he who understands form, and the manner of superinducing this yellowness, gravity, ductility, fixedness, faculty of fusion, solution, &c., with their particular degrees and proportions, will consider how to join them together in some body, so that a transmutation into gold shall follow.'

On the other hand, the analytic method, or, 'the enquiry from what origin gold or any other metal or stone is generated from its first fluid matter or rudiments, up to a perfect mineral,' is to be perceived by what Bacon calls the latent process, or a search for 'what in every generation or transformation of bodies, flies off, what remains behind, what is added, what separated, &c.; also, in other alterations and motions, what gives motion, what governs it, and the like.' Bacon appears to have thought that qualities separate from the substances themselves were attainable, and if not capable of physical isolation, were at all events capable of physical transference and superinduction.

Subsequently to Bacon a belief has generally existed, and now to a great extent exists, in what are called secondary causes, or consequential steps, wherein one phenomenon is supposed necessarily to hang on another, and that on another, until at last we arrive at an essential cause, subject immediately to the First Cause. This notion is generally prevalent both on the Continent and in this country: nothing is more familiar than the expression 'study the effects in order to arrive at the causes.'

Instead of regarding the proper object of physical science

as a search after essential causes, I believe it ought to be, and must be, a search after facts and relations—that although the word Cause may be used in a secondary and concrete sense, as meaning antecedent forces, yet in an abstract sense it is totally inapplicable : we cannot predicate of any physical agency that it is abstractedly the cause of another ; and if, for the sake of convenience, the language of secondary causation be permissible, it should be with reference only to the special phenomenon referred to, as it can never be generalised,

The misuse, or rather varied use, of the term Cause, has been a source of great confusion in physical theories, and philosophers are even now by no means agreed as to their conception of causation. The most generally received view of causation, that of Hume, refers it to invariable antecedence,—*i.e.* we call that a cause which invariably precedes, that an effect which invariably succeeds. Many instances of invariable sequence might, however, be selected, which do not present the relation of cause and effect : thus, as Reid observes, and Brown does not satisfactorily answer, day invariably precedes night, and yet day is not the cause of night. The seed, again, precedes the plant, but is not the cause of it ; so that when we study physical phenomena it becomes difficult to separate the idea of causation from that of force, and these have been regarded as identical by some philosophers. To take an example which will contrast these two views : if a floodgate be raised, the water flows out ; in ordinary parlance, the water is said to flow *because* the floodgate is raised : the sequence is invariable ; no floodgate, properly so called, can be raised without the water flowing out, and yet in another, and perhaps more strict, sense, it is the gravitation of the water which *causes* it to flow. But though we may truly say that, in this instance, gravitation causes the water to flow, we cannot in truth abstract the proposition, and say, generally, that gravitation is the cause of water flowing, as water may flow from other causes, gaseous elasticity for instance, which will cause water to flow from a receiver full of air into one that is exhausted ; gravitation may also, under certain circumstances, arrest instead of cause the flow of water.

Upon neither view, however, can we get at anything like

abstract causation. If we regard causation as invariable sequence, we can find no case in which a given antecedent is the only antecedent to a given sequent: thus, if water could flow from no other cause than the withdrawal of a floodgate, we might say abstractedly that this was the cause of water flowing. If, again, adopting the view which looks to causation as a force, we could say that water could be caused to flow only by gravitation, we might say abstractedly that gravitation was the cause of water flowing—but this we cannot say; and if we seek and examine any other example, we shall find that causation is only predicable of it in the particular case, and cannot be supported as an abstract proposition; yet this is constantly attempted. Nevertheless, in each *particular* case where we speak of Cause, we habitually refer to some antecedent power or force: we never see motion or any change in matter take effect without regarding it as produced by some previous change; and, when we cannot trace it to its antecedent, we mentally refer it to one; but whether this habit be philosophically correct may be disputed. In other words, it seems questionable, not only whether cause and effect are convertible terms with antecedence and sequence, but whether in fact cause does precede effect, whether force does precede the change in matter of which it is said to be the cause.

The actual priority of cause to effect has been doubted, and their simultaneity argued with much ability. As an instance of this argument it may be said, the attraction which causes iron to approach the magnet is simultaneous with and ever accompanies the movement of the iron; the movement is evidence of the co-existing cause or force, but there is no evidence of any interval in time between the one and the other. On this view time would cease to be a necessary element in causation; the idea of cause, except perhaps as referred to a primeval creation, would cease to exist; and the same arguments which apply to the simultaneity of cause with effect would apply to the simultaneity of Force with Motion. We could not, however, even if we adopted this view, dispense with the element of time in the sequence of phenomena; the effect being thus regarded as ever accompanied simultaneously by its appropriate cause, we should still refer to some

antecedent effect; and our reasoning as applied to the successive production of all natural changes would be the same.

Habit and the identification of thoughts with phenomena so compel the use of recognised terms, that we cannot avoid using the word 'cause' even in the sense in which objection is taken; and if we struck it out of our vocabulary, our language, in speaking of successive changes, would be unintelligible to the present generation. The common error, if I am right in supposing it to be such, consists in the abstraction of cause, and in supposing in each case a general secondary cause—a something which is not the first cause, but which, if we examine it carefully, must have all the attributes of a first cause, and an existence independent of, and dominant over, matter.

The relations of electricity and magnetism afford us a very instructive example of the belief in secondary causation. Subsequent to the discovery by Oersted of electro-magnetism, and prior to that by Faraday of magneto-electricity, electricity and magnetism were believed by the highest authorities to stand in the relation of cause and effect—*i.e.* electricity was regarded as the cause, and magnetism as the effect; and where magnets existed without any apparent electrical currents to cause their magnetism, hypothetical currents were supposed, for the purpose of carrying out the causative view; but magnetism may now be said with equal truth to be the cause of electricity, and electrical currents may be referred to hypothetical magnetic lines: if therefore electricity cause magnetism, and magnetism cause electricity, why then electricity causes electricity, which becomes, so as to speak, a *reductio ad absurdum* of the doctrine.

To take another instance, which may render these positions more intelligible. By heating bars of bismuth and antimony in contact, a current of electricity is produced; and if their extremities be united by a fine wire, the wire is heated. Now, here the electricity in the metals is said to be caused by heat, and the heat in the wire to be caused by electricity, and in a concrete sense this is true; but can we thence say abstractedly that heat is the cause of electricity, or that electricity is the cause of heat? Certainly not; for if either be

true, both must be so, and the effect then becomes the cause of the cause, or, in other words, a thing causes itself. Any other proposition on this subject will be found to involve similar difficulties, until, at length, the mind will become convinced that abstract secondary causation does not exist, and that a search after essential causes is vain.

The position which I seek to establish in this Essay is, that the various affections of matter which constitute the main objects of experimental physics, viz. heat, light, electricity, magnetism, chemical affinity, and motion, are all correlative, or have a reciprocal dependence; that neither, taken abstractedly, can be said to be the essential cause of the others, but that either may produce or be convertible into, any of the others: thus heat may mediately or immediately produce electricity, electricity may produce heat; and so of the rest, each merging itself as the force it produces becomes developed: and that the same must hold good of other forces, it being an irresistible inference from observed phenomena that a force cannot originate otherwise than by devolution from some pre-existing force or forces.

The term force, although used in very different senses by different authors, in its limited sense may be defined as that which produces or resists motion. Although strongly inclined to believe that the other affections of matter, which I have above named, are and will ultimately be resolved into modes of motion, many arguments for which view will be given in subsequent parts of this Essay, it would be going too far, at present, to assume their identity with it; I therefore use the term force in reference to them, as meaning that active principle inseparable from matter which is supposed to induce its various changes.

The word force and the idea it aims at expressing, might be objected to by the purely physical philosopher on similar grounds to those which apply to the word cause, as it represents a subtle mental conception, and not a sensuous perception or phenomenon. The objection would take something of this form. If the string of a bent bow be cut, the bow will straighten itself; we thence say there is an elastic *force* in the bow which straightens it; but if we applied our expressions to

this experiment alone, the use of the term force would be superfluous, and would not add to our knowledge on the subject. All the information which our minds could get would be as sufficiently obtained from the expression, when the string is cut the bow becomes straight, as from the expression, the bow becomes straight by its elastic force. Do we know more of the phenomenon, viewed without reference to other phenomena, by saying it is produced by force? Certainly not. All we know or see is the effect; we do not see force—we see motion or moving matter.

If now we take a piece of caoutchouc and stretch it, when released it returns to its original length. Here, though the subject-matter is very different, we see some analogy in the effect or phenomenon to that of the strung bow. If, again, we suspend an apple by a string, cut the string, the apple falls. Here, though it is less striking, there is still an analogy to the strung bow and the caoutchouc.

Now, when the word force is employed as comprehending these three different phenomena, we find some use in the term, not by its explaining or rendering more intelligible the *modus agendi* of matter, but as conveying to the mind something which is alike in the three phenomena, however distinct they may be in other respects: the word becomes an abstract or generalised expression, and regarded in this light is of high utility. Although I have given only three examples, it is obvious that the term would equally apply to 300 or 3,000 cases.

But it will be said, the term force is used not as expressing the effect, but as that which produces the effect. This is true, and in this its ordinary sense I shall use it in these pages. But though the term has thus a potential meaning, to depart from which would render language unintelligible, we must guard against supposing that we know essentially more of the phenomena by saying they are produced by something, which something is only a word derived from the constancy and similarity of the phenomena we seek to explain by it. The relations of the phenomena to which the terms force or forces are applied give us real knowledge; these relations may be called relations of forces; our knowledge of them is not

thereby lessened, and the convenience of expression is greatly increased, but the separate phenomena are not more intimately known; no further insight into why the apple falls is acquired by saying it is forced to fall, or it falls by the force of gravitation; by the latter expression we are enabled to relate it most usefully to other phenomena, but we still know no more of the particular phenomenon than that under certain circumstances the apple does fall.

In the above illustrations force has been treated as the producer of motion, in which case the evidence of the force is the motion produced; thus we estimate the force used to project a cannon-ball in terms of the mass of matter and the velocity with which it is projected. The evidence of force when the term is applied to resistance to motion is of a somewhat different character; the matter resisting is molecularly affected, and has its structure more or less changed; thus a strip of caoutchouc to which a weight is suspended is elongated, and its molecules are displaced as compared with their position when unaffected by the gravitating force. So a piece of glass bent by an appended weight has its whole structure changed; this internal change is made evident by transmitting through it a beam of polarised light; a relation thus becomes established between the molecular state of bodies and the external forces or motion of masses. Every particle of the caoutchouc or glass must be acting and contributing to resist or arrest the motion of the mass of matter appended to it.

We need some word to express this state of tension; we know that it produces an effect, though the effect be negative in character: although in this effort of inanimate matter we can no more trace the mode of action to its ultimate elements than we can follow out the connection of our own muscles with the volition which calls them into action, we are experimentally convinced that matter changes its state by the agency of other matter, and this agency we call force.

In placing the weight on the glass, we have moved the former to an extent equivalent to that which it would again describe if the resistance were removed, and this motion of the mass becomes an exponent or measure of the force exerted

on the glass ; while this is in the state of tension, the force is ever existing, capable of reproducing the original motion, and while in a state of abeyance as to actual motion, it is really acting on the glass. The motion is suspended, but the force is not annihilated.

But it may be objected, if tension or static force be thus motion in abeyance, there is at all times a large amount of dynamical action subtracted from the universe. Every stone raised and left upon a hill, every spring that is bent, and has required force to upraise or bend it, has for a time, and possibly for ever, withdrawn this force, and annihilated it. Not so ; when we raise a weight and leave it at the point to which it has been elevated we have changed the centre of gravity of the earth, and consequently the earth's position with reference to the sun, planets, and stars ; the effort we have made pervades and shakes the universe ; nor can we present to the mind any exercise of force, which is not thus permanent in its dynamical effects. If, instead of one weight being raised, we raise two weights, each placed at points of the earth diametrically opposite each other, it would be said, here you have compensation, a balance, no change in the centre of gravity of the earth ; but we have increased the mean diameter of the earth, and a perturbation of our planet, and of all other celestial bodies, necessarily ensues.

The force may be said to be in abeyance with reference to the effect it would have produced, if not arrested, or placed in a state of tension ; but in the act of imposing this state, the relations of equilibrium with other bodies have been changed, and these move in their turn, so that motion of the same amount would seem to be ever affecting matter conceived in its totality.

Press the hands violently together ; the first notion may be that this is power locked up, and that no change ensues. Not so ; the blood courses more quickly, respiration is accelerated, changes, which we may not be able to trace, take place in the muscles and nerves, transpiration is increased ; we have given off force in various ways, and must, if such efforts be prolonged, replenish our sources of power, by fresh chemical action in the stomach.

In books which treat of statics and dynamics, it is common and perhaps necessary to isolate the subjects of consideration; to suppose, for instance, two bodies gravitating, and to ignore the rest of the universe. But no such isolation exists in reality, nor could we predict the result if it did exist. Would two bodies gravitate towards each other in empty space, if space can be empty? The notion that they would is founded on the theory of attraction, which Newton himself repudiated, further than as a convenient means of regarding the subject. For purposes of instruction or argument it may be convenient to assume isolated matter: many conclusions so arrived at may be true, but many will be erroneous.

If, in producing effects of tension or of static force, the effort made pervades the universe, it may be said, when the bent spring is freed, when the raised weight falls, a converse series of motions must be effected, and this theory would lead to a mere reciprocation, which would be equally unproductive of permanent change with the annihilation of force. If raising the weight has changed the centre of gravity of the earth, and thence of the universe, the fall of the weight, it will be said, restores the original centre of gravity, and everything comes back to its original status. In this argument we again, in thought, isolate our experiment; we neglect surrounding circumstances. Between the time of the raising and falling of the weight, be the interval never so small, nay more, during the rising and during the fall, the earth has been going on revolving round its axis and round the sun, to say nothing of other changes, such as temperature, cosmical magnetism, &c., which we may call accidental, but which, if we knew all, would probably be found to be as necessary and as reducible to law as the motion of the earth. A change having taken place, the fall of the weight does not bring back the *status quo*, but other changes supervene, and so on. Nothing repeats itself, because nothing can be placed again in the same condition: the past is irrevocable.

MOTION.

MOTION—which has been taken as the main exponent of force in the above examples—is the most obvious, the most distinctly conceived of all the affections of matter. Visible motion, or relative change of position in space, is a phenomenon so obvious to simple apprehension, that to attempt to define it would be to render it more obscure; but with motion, as with all physical appearances, there are certain vanishing gradations or undefined limits, at which the obvious mode of action fades away; to detect the continuing existence of the phenomena we are obliged to have recourse to other than ordinary methods of investigation, and we frequently apply other and different names to the effects so recognised.

Thus sound is motion; and although in the earlier periods of philosophy the identity of sound and motion was not traced out, and they were considered distinct affections of matter—indeed, at the close of the last century a theory was advanced that sound was transmitted by the vibrations of an ether—we now so readily resolve sound into motion, that to those who are familiar with acoustics, the phenomena of sound immediately present to the mind the idea of motion, *i.e.* motion of ordinary matter.

Again, with regard to light: no doubt now exists that light moves or is accompanied by motion. Here the phenomena of motion are not made evident by the ordinary sensuous perception, as, for instance, the motion of a visibly moving projectile would be, but by an inverse deduction from known relations of motion to time and space: as all observation teaches us that bodies in moving from one point in space to another occupy time, we conclude that, wherever a continuing phenomenon is rendered evident in two different points of space at different times, there is motion, though we cannot see the

progression. A similar deduction convinces us of the motion of electricity.

As we in common parlance speak of sound moving, although sound is motion, it requires no great stretch of imagination to conceive light and electricity as motions, and not as things of moving. If one end of a long bar metal be struck, a sound is soon perceptible at the other end. This we now know to be a vibration of the bar ; sound is but a word expressive of the mode of motion impressed on the bar ; so one end of a column of air or glass subjected to a luminous impulse gives a perceptible effect of light at the other end : this can equally be conceived to be a vibration or transmitted motion of particles in the transparent column : this question will, however, be further discussed hereafter ; for the present we will confine ourselves to motion within the limits to which the term is usually restricted.

With the perceptible phenomena of motion the mental conception has been invariably associated to which I have before alluded, and to which the term force is given—the which conception, when we analyse it, refers us to some antecedent motion. If we except the production of motion by heat, light, &c., which will be considered in the sequel, when we see a body moving we look to motion having been communicated to it by matter which has previously moved.

Of absolute rest Nature gives us no evidence : all matter, as far as we can ascertain, is ever in movement, not merely in masses, as with the planetary spheres, but also molecularly, or throughout its most intimate structure : thus every alteration of temperature produces a molecular change throughout the whole substance heated or cooled ; slow chemical or electrical actions, actions of light or invisible radiant forces, are always at play, so that as a fact we cannot predicate of any portion of matter that it is absolutely at rest. Supposing, however, that motion is not an indispensable function of matter, but that matter can be at rest, matter at rest would never of itself cease to be at rest ; it would not move unless impelled to such motion by some other moving body, or body which has moved. This proposition applies not merely to impulsive motion, as when a ball at rest is struck by a moving

body, or pressed by a spring which has previously been moved, but to motion caused by attractions such as magnetism or gravitation. Suppose a piece of iron at rest in contact with a magnet at rest; if it be desired to move the iron by the attraction of the magnet, the magnet or the iron must first be moved; so before a body falls it must first be raised. A body at rest would therefore continue so for ever, and a body once in motion would continue so for ever, in the same direction and with the same velocity, unless impeded by some other body, or affected by some other force than that which originally impelled it. These propositions may seem somewhat arbitrary, and it has been doubted whether they are necessary truths; they have for a long time been received as axioms, and there can at all events be no harm in accepting them as postulates. It is however very generally believed that if the visible or palpable motion of one body be arrested by its impact on another body, the motion ceases, and the force which produced it is annihilated.

Now, the view which I venture to submit is, that force cannot be annihilated, but is merely subdivided or altered in direction or character. First, as to direction. Wave your hand: the motion, which has apparently ceased, is taken up by the air, from the air by the walls of the room, &c., and so by direct and reacting waves, continually comminuted, but never destroyed. It is true that, at a certain point, we lose all means of detecting the motion, from its minute subdivision, which defies our most delicate means of appreciation, but we can indefinitely extend our power of detecting it accordingly as we confine its direction, or increase the delicacy of our examination. Thus, if the hand be moved in unconfined air, the motion of the air would not be sensible to a person at a few feet distance; but if a piston of the same extent of surface as the hand be moved with the same rapidity in a tube, the blast of air may be distinctly felt at several yards' distance. There is no greater absolute amount of motion in the air in the second than in the first case, but its direction is restrained, so as to make the means of detection more facile. By carrying on this restraint, as in the air-gun, we get a power of detecting the motion, and of moving other bodies at far greater

distances. The puff of air which would in the air-gun project a bullet a quarter of a mile, if allowed to escape without its direction being restrained, as by the bursting of a bladder, would not be perceptible at a yard distance, though the same absolute amount of motion be in both the cases impressed on the surrounding air.

It may, however, be asked, what becomes of force when motion is arrested or impeded by the counter-motion of another body? This is generally believed to produce rest, or entire destruction of motion, and consequent annihilation of force: so indeed it may, as regards the motion of the masses, but a new force, or new character of force, now ensues, the exponent of which, instead of visible motion, is heat. I venture to regard the heat which results from friction or percussion as a continuation of the force which was previously associated with the moving body, and which, when this impinges on another body, ceasing to exist as gross, palpable motion, continues to exist as heat.

Thus, let two bodies, A and B, be supposed to move in opposite directions (putting for the moment out of the question all resistance, such as that of the air, &c.), if they pass each other without contact each will move on for ever in its respective direction with the original velocity, but if they touch each other the velocity of the movement of each is reduced, and each becomes heated: if this contact be slight, or such as to occasion but a slight diminution of their velocity, as when the surfaces of the bodies are oiled, then the heat is slight; but if the contact be such as to occasion a great diminution of motion, as in percussion, or as when the surfaces are roughened, then the heat is great, so that in all cases the resulting heat is proportionate to the diminished velocity. Where, instead of resisting and consequently impeding the motion of the body A, the body B gives way, or itself takes up the motion originally communicated to A, then we have less heat in proportion to the motion of the body B, for here the operation of the force continues in the form of palpable motion: thus the heat resulting from friction in the axle of a wheel is lessened by surrounding it by rollers; these take up the primary motion of the axle, and the less, by this means,

the initial motion is impeded, the less is the resulting heat. Again, if a body move in a fluid, although some heat is produced, the heat is apparently trifling, because the particles of the fluid themselves move, and continue the motion originally communicated to the moving body: for every portion of motion communicated to them this loses an equivalent, and where both lose, then an equivalent of heat results.

As the converse of this proposition, it should follow that the more rigid the bodies impinging on each other the greater should be the amount of heat developed by friction, and so we find it. Flint, steel, hard stones, glass, and metals are those bodies which give the greatest amount of heat from friction or percussion; while water, oil, &c. give little or no heat, and from the ready mobility of their particles lessen its development when they are interposed between rigid moving bodies. Thus, if we oil the axles of wheels, we have more rapid motion of the bodies themselves, but less heat; if we increase the resistance to motion, as by roughening the points of contact, so that each particle strikes against and impedes the motion of others, then we have diminished motion, but increased heat; or if the bodies be smooth, but instead of sliding past each other be pressed closely together and then rubbed, we shall in many cases evolve more heat than by the roughened bodies, as we get a greater number of particles in contact and a greater resistance to the initial motion. I cannot present to my mind any case of heat resulting from friction which is not explicable by this view: friction, according to it, is simply impeded motion. The greater the impediment, the more force is required to overcome it, and the greater is the resulting heat; this resulting heat being a continuation of indestructible force, capable, as we shall presently see, of reproducing palpable motion, or motion of definite masses.

Whatever be the nature of the bodies, rough or smooth, solid or liquid, provided there be the same initial force, and the whole motion be ultimately arrested, there should be the same amount of heat developed, though where the motion is carried on through a great number of points of matter we do not so sensibly perceive the resulting heat from its greater dissipation. The friction of fluids produces heat, an effect first

noticed, I believe, by Mayer. The total heat produced by the friction of fluids should, therefore, it will be said, be equal to that produced by the friction of solids; for although each particle produces little heat, the motion being readily taken up by the neighbouring particles, yet by the time the whole mass has attained a state of rest there has been the same impeding of the initial motion as by the friction of solids if produced by the same initial force. If the heat be viewed in the aggregate, and allowance be made for the specific thermal capacity of the substances employed, it probably is the same, though apparently less; the heat in the case of solids being manifested at certain defined points, while in that of fluids it is dissipated, both the time and space during and through which the motion is propagated differ in the two cases, so that the heat in the latter case is more readily carried off by surrounding bodies.

If the body be elastic, and by its reaction the motion impressed on it by the initial force be continued, then the heat is proportionately less; and were a substance perfectly elastic and no resistance opposed to it by the air or other matter, then the movement once impressed would be perpetual, and no heat would result. A ball of caoutchouc bandied about for many minutes between a racket and a wall is not perceptibly heated, while a leaden bullet projected by a gun against a wall is rendered so hot as to be intolerable to the touch: in the former case, the motion of the mass is continued by the reaction due to its elasticity; in the latter the motion of the mass is extinguished and heat ensues.

A pendulum started in the exhausted receiver of an air-pump continues its oscillation for hours or even days; the friction at its point of suspension and the resistance of the air is minimised, and the heat is therefore imperceptible, but these trifling resistances in the end arrest the motion of the mass, the one giving it out as heat, the other conveying the force to the receiver, and thence to surrounding bodies. Similar reasoning may be applied to the oscillation of a coiled spring and balance-wheel.

To wind up a clock a certain amount of force is expended by the muscles; this force is given back by the descent of

the weight, the wheels move, the pendulum is kept oscillating, heat is generated at each point of friction, and the surrounding air is set in motion, a part of which is made obvious to us by the ticking sound. But it will be said, if instead of allowing the weight to act upon the machinery, the cord by which it is suspended be cut, the weight drops and the force is at an end. By no means, for in this case the house is shaken by the concussion, and thus the force and motion are continued, while in the former case the weight reaches the ground quietly, and no evidence of force or motion is manifested by its impact, the whole having been previously dissipated.

If the initial motion, instead of being arrested by the impact of other bodies, as in friction or percussion, is impeded by confinement or compression, as where the dilatation of a gas is prevented by mechanical means, heat equally results: thus if a piston is used to compress air in a closed vessel, the compressed air and, from it, the sides of the vessel, will be heated: the air being unable to take up and carry on the original motion, communicates molecular motion or expansion to all bodies in contact with it; and, conversely, if we expand air by mechanical motion, as by withdrawing the piston, cold is produced. So when a solid has its particles compressed or brought nearer together, as when a bar of iron is hammered, heat is produced beyond that which is due to percussion alone. In this latter case we cannot very easily effect the converse result, or produce cold by the mechanical dilatation of a solid, though the phenomena of solution, where the particles of a solid are detached from each other, or drawn more widely asunder, give us an approximation to it: in the case of solution cold is produced.

We are from a very extensive range of observation and experiment entitled to conclude that, with some curious exceptions to be presently noticed, whenever a body is compressed or brought into smaller dimensions it is heated, *i.e.* it expands neighbouring substances. Whenever it is dilated or increased in volume it is cooled, or contracts neighbouring substances.

Mr. Joule has made several experiments for the purpose of ascertaining what quantity of heat is produced

by a given mechanical action. His mode of experimenting is as follows: An apparatus formed of floats or paddles of brass or iron is made to rotate in a bath of water or mercury. The power which gives rise to this rotation is a weight raised like a clock-weight to a certain height; this by acting during its fall on a spindle and pulley communicates motion to the paddle-wheel, the water or mercury serving as a friction medium and calorimeter; and the heat is measured by a delicate mercurial thermometer. The results of his experiments he considers prove that a fall of 772 lbs. through a space of one foot is able to raise the temperature of one pound of water through one degree of Fahrenheit's thermometer. Mr. Joule's experiments are of extreme delicacy—he tabulates to the thousandth part of a degree of Fahrenheit, and a large number of his thermometric data are comprehended within the limits of a single degree. Other experimenters have given very different numerical results, but the general opinion seems to be that the numbers given by Mr. Joule are the nearest approximation to the truth yet obtained.

Hitherto I have made no distinction as to the physical character of the bodies impinging on each other; but Nature gives us a remarkable difference in the character or mode of the force eliminated by friction, accordingly as the bodies which impinge are homogeneous or heterogeneous: if the former, heat alone is produced; if the latter, electricity.

We find, indeed, instances given by authors of electricity resulting from the friction of homogeneous bodies; but, as I stated in my original Lectures, I have not found such facts confirmed by my own experiments, and this conclusion has been corroborated by some experiments of Professor Erman, communicated to the meeting of the British Association in the year 1845, in which he found that no electricity resulted from the friction of perfectly homogeneous substances; as, for instance, the ends of a broken bar. Such experiments as these will, indeed, be seldom free from slight electrical currents, on account of the practical difficulty of fulfilling the condition of perfect homogeneity in the substances themselves, their size, their temperature, &c.; but the effects produced are very trifling and vary in direction, and the resultant effect

is nought. Indeed, it would be difficult to conceive the contrary. How could we possibly image to the mind or describe the direction of a current from the same body to the same body, or give instructions for a repetition of the experiment? It would be unintelligible to say that in rubbing to and fro two pieces of bismuth, iron, or glass a current of electricity circulated from bismuth to bismuth, or from iron to iron, or from glass to glass; for the question immediately occurs—from which bismuth to which does it circulate? And should this question be answered by calling one piece A, and the other B, this would only apply to the particular specimens employed, the distinctive appellation denoting a distinction in fact, as otherwise A could be substituted for B, and the bar to which the positive electricity flowed would in turn become the bar to which the negative electricity flowed. We may say that it circulates from rough glass to smooth, from cast iron to wrought, for here there is not homogeneity. It is moreover conceivable, though not experimentally proved, that when the motion is continuous in a definite direction, electricity may result from the friction of homogeneous bodies. If A and B rub against each other, revolving in opposite directions, concentric currents of positive and negative electricity may be conceived circulating within the metals, and be described by reference to the direction of their motion; this indeed would be a different phenomenon from those we have been considering; but without some distinction between the two substances in quality or direction, the electrical effects are indescribable, if not inconceivable.

When, however, homogeneous bodies are fractured or even rubbed together, phenomena are observed to which the term electricity is applied; a flash or line of light appears at the point of friction, which by some is called electrical, by others phosphorescent.

I have myself observed a remarkable case of the kind in the caoutchouc fabric now commonly used for waterproof clothing: if two folds of this substance be allowed to cohere so as partly to unite and present a difficulty of separation, then, on stripping the one from the other, or tearing them asunder, a line of light will follow the line of separation.

If this class of phenomena be electrical, it is electricity determined as it is generated; there is no dual character impressed on the matter acting, the flash is electrical, as a spark from the percussion of flint is electrical, or as the slow combustion of phosphorus, or any other case of the development of heat and light. It seems to be better to class this phenomenon under the categories of heat and light than under that of electricity, the latter word being retained for those cases where a dual or polar character of force is manifested. In experiments which have been made by the friction of similar substances where the one appears positively and the other negatively electrical, there will be found some difference in the mode of rubbing, by which the molecular state of the bodies is in all probability changed, making one a dissimilar substance from the ether; thus it is said by Bergmann, that when two pieces of glass are rubbed so that all the parts of one pass over one part of the other, the former is positive and the latter negative. It is obvious that in this case the rubbing in one is confined to a line, and that must be more altered in molecular structure at the line of friction than the one where the friction is spread over the whole surface: so if a ribbon be drawn transversely over another ribbon, the substances are not, *qua* the rubbing action, identical; so again, in the rupture of crystals, we are dealing with substances having a polar arrangement of particles—the surfaces of the fragments cannot be assumed to be molecularly identical.

The development of electricity by the common electrical machine arises, as far as I can understand it, from the separation or rupture of contiguity between dissimilar bodies; a metallic surface, the amalgam of the cushion, is in contact with glass; these two bodies act upon each other by the force of cohesion; and when, by an external mechanical force, this is ruptured, as it is at each moment of the motion of the glass plate or cylinder, electricity is developed in each: were they similar bodies, heat only would be developed.

According to the experiments of Mr. Sullivan electricity may be produced by vibration alone if the substance vibrating be composed either of dissimilar metals, as a wire partly of

iron and partly of brass, caused to emit a musical sound, or of the same metal if its parts be not homogeneous, as a piece of iron one portion of which is hard and crystallised, and the other soft and fibrous; the current resulting appears to be due to the vibration, and not to heat engendered, as it ceases immediately with the vibration.

I venture therefore to think that in our present state of knowledge, where the mutually impinging bodies are homogeneous, heat and not electricity is the result of friction and percussion; where the bodies impinging are heterogeneous, we may safely state that electricity is always produced by friction or percussion, although heat in a greater or less degree accompanies it; but when we come to the question of the ratio in which frictional electricity is produced, as determined by the different characters of the substances employed, we find very complex results. Bodies may differ in so many particulars which influence more or less the development of electricity, such as their chemical constitution, the state of their surfaces, their state of aggregation, their transparency or opacity, their power of conducting electricity, &c., that the *normæ* of their action are very difficult of attainment. As a general rule, it may be said that the development of electricity is greater when the substances employed are broadly distinct in their physical and chemical qualities, and more particularly in their conducting powers; but up to the present time the laws governing such development have not been even approximately determined.

I have said, in reference to the various forces or affections of matter, that either of them may, *mediately* or *immediately*, produce the others; and this is all I can venture to predicate of them in the present state of science. Where at present no immediate relation is established between any of them, electricity generally forms the intervening link.

Motion, then, will directly produce *heat* and *electricity*, and electricity, being produced by it, will produce *magnetism*—a force which is always developed by electrical currents, at right angles to the direction of those currents, as will be subsequently more fully explained. *Light* also is readily produced, to all appearance, by motion, directly, when accompanying the

heat of friction, or mediately, by electricity resulting from motion, as in the electrical spark, which has most of the attributes of solar light, differing from it only in those respects in which light differs when emanating from different sources or seen through different media; for instance, in the position of the fixed lines in the spectrum or in the ratios of the spaces occupied by rays of different refrangibility. In the decompositions and compositions which the terminal points proceeding from the conductors of an electrical machine develope when immersed in different chemical media, we get the production of *chemical affinity* by electricity, of which motion is the initial source. Lastly, *motion* may be again reproduced by the forces which have emanated from motion; thus, the divergence of the electrometer, the revolution of the electrical wheel, the deflection of the magnetic needle, are, when resulting from frictional electricity, palpable movements reproduced by the intermediate modes of force, which have themselves been originated by motion.

HEAT.

If we now take Heat as our starting-point, we shall find that the other modes of force may be readily produced by it. To take motion first: this is so generally, I think I may say invariably, the immediate effect of heat, that we may almost, if not entirely, resolve heat into motion, and view it as a mechanically repulsive force, a force antagonistic to attraction of cohesion or aggregation, and tending to *move* the particles of all bodies, or to separate them from each other.

It may be well here to premise, that in using the terms 'particles' or 'molecules,' which will be frequently employed in this Essay, I do not use them in the sense of the atomist, or mean to assert that matter consists of indivisible particles or atoms. By many who adopt the atomic doctrine the term 'molecule' is used to signify a definite congeries of atoms, forming an integral constituent of matter, somewhat as a brick may be said to be a congeries of sand atoms, but a structural element of a house. The word 'molecule' will be used by me for the necessary purpose of contradistinguishing the action of the indefinitely minute physical elements of matter from that of masses having a sensible magnitude, much in the same way as the terms 'lines' or 'points' are used in an abstract sense; though there does not exist, in fact, a thing which has length and breadth without thickness, and though a thing without parts or dimensions is nothing.

If we put aside the sensation which heat produces in our own bodies, and regard heat simply in relation to its effect upon inorganic matter, we find that, with a very few exceptions, which I shall presently notice, the effects of what is called heat are simply an expansion of the matter acted upon, and that the matter so expanded has the power by its own contraction of communicating expansion to all bodies in conti-

guity with it. Thus, if the body be a solid: for instance, iron; a liquid, say water; or a gas, say atmospheric air—each of these, when heated, is expanded in every direction: in the two former cases, by increasing the heat to a certain point, we change the physical character of the substance, the solid becomes a liquid, and the liquid becomes a gas; these, however, are still expansions, particularly the latter, when, at a certain period, the expansion becomes rapidly and indefinitely greater. But what is, in fact, commonly done in order to heat a substance, or to increase the heat of a substance? It is merely approximated to some other heated, that is, to some other expanded substance, which latter is cooled or contracted as the former expands. Let us now divest the mind of the impression that heat is in itself anything substantive, and suppose that these phenomena are regarded for the first time, and without any preconceived notions on the subject; let us introduce no hypothesis, but merely express as simply as we can the facts of which we have become cognisant; to what do they amount? To this, that matter has pertaining to it a molecular repulsive power, a power of dilatation, which is communicable by continuity or proximity.

Heat thus viewed, is motion, and this molecular motion we may readily change into the motion of masses, or motion in its most ordinary and palpable form: for example, in the steam-engine, the piston and all its concomitant masses of matter are moved by the molecular dilatation of the vapour of water.

To produce continuous motion there must be an alternate action of heat and cold; a given portion of air, for instance, heated beyond the temperature of the circumambient air, is expanded. If now it be made to act on a movable piston, it moves this to a point at which the tension or elastic force of the confined air equals that of the surrounding air. If the confined air be kept at this point, the piston would remain stationary; but if it be cooled, the external air exercising then a greater relative degree of pressure, the piston returns towards its original position; just as it will be seen, when we come to the magnetic force, that a magnet placed in a particular position produces motion in iron near it; but to make

this motion continuous, or to obtain an available mechanical power, the magnet must be demagnetised, or else a stable equilibrium is obtained.

In the case of the piston moved by heated air, the motion of the mass becomes the exponent of the amount of heat—*i.e.* of the expansion or separation of the molecules; and we do not, by any of our ordinary methods, test heat in any other way than by its purely dynamical action. The various modifications of the thermometer and pyrometer are all measurers of heat by motion; in these instruments liquid or solid bodies are expanded and elongated, *i.e.* moved in a definite direction, and, either by their own visible motion, or by the motion of an attached index, communicate to our senses the amount of the force by which they are moved. There are, indeed, some delicate experiments which tend to prove that a repulsive action between separate masses is produced by heat. Fresnel thought he had proved that mobile bodies heated in an exhausted receiver repelled each other to sensible distances; but his experiments have not been confirmed; and Baden Powell found that the coloured rings usually called Newton's rings change their breadth and position, when the glasses between which they appear are heated, in a manner which showed that the glasses repelled each other. M. Faye's theory of comets is based on some such repellent force. There is, however, some difficulty in presenting these phenomena to the mind in the same aspect as the molecular repulsive action of heat.

The phenomena of what is termed latent heat have been generally considered as strongly in favour of that view which regards heat either as actual matter, or, at all events, as a substantive entity, and not a motion or affection of ordinary matter.

The hypothesis of latent matter is, I venture with diffidence to think, a dangerous one—it is something like the old principle of Phlogiston: it is not tangible, visible, audible; it is, in fact, a mere subtle mental conception, and ought, I submit, only to be received on the ground of absolute necessity, the more so as these subtleties are apt to be carried on to other natural phenomena, and so they add to the hypothetical

scaffolding which is seldom requisite, and should be sparingly used, even in the early stages of discovery. As an instance I think a striking one, of the injurious effects of this, I will mention the analogous doctrine of 'invisible light;' and I do this meaning no disrespect to its distinguished author, any more than, in discussing the doctrine of latent heat, I can be supposed, in the slightest degree, to aim at detracting from the merits of the illustrious investigators of the facts which that doctrine seeks to explain. Is not 'invisible light' a contradiction in terms? Has not light ever been regarded as that agent which affects our visual organs? Invisible light, then, is darkness, and if it exist, then is darkness light. I know it may be said, that one eye can detect light where another cannot; that a cat may see where a man cannot; that an insect may see where a cat cannot; but then it is not invisible light to those who see it: the light, or rather the object seen by the cat, may be invisible to the man, but it is visible to the cat, and, therefore, cannot abstractedly be said to be invisible. If we go further, and find an agent which affects certain substances similarly to light, but does not, as far as we are aware, affect the visual organs of any animal, then is it not an erroneous nomenclature which calls such an agent light? There are many cases in which a deviation from the once accepted meaning of words has so gradually entered into common usage as to be unavoidable, but I cannot but think that additions to such cases should as far as possible be avoided, as injurious to that precision of language which is one of the safest guards to knowledge, and from the absence of which physical science has materially suffered.

Let us now shortly examine the question of latent heat, and see whether the phenomena may not be as well, if not more satisfactorily, explained without the hypothesis of latent matter, an idea presenting many similar difficulties to that of invisible light, though more sanctioned by usage. Latent heat is supposed to be the matter of heat, associated, in a masked or dormant state, with ordinary matter, not capable of being detected by any test so long as the matter with which it is associated remains in the same physical state, but communicated to or absorbed from other bodies, when the

matter with which it is associated changes its state. To take a common example: a pound or given weight of water at 172°, mixed with an equal weight of water at 32°, will acquire a mean temperature, or 102°; while water at 172°, mixed with an equal weight of ice at 32°, will be reduced to 32°. By the theory of latent heat this phenomenon is thus explained: In the first case, that of the mixture of water with water, both the bodies being in the same physical state, no latent heat is rendered sensible, or sensible heat latent; but in the second, the ice changing its condition from the solid to the liquid state, abstracts from the liquid as much heat as it requires to maintain it in the liquid state, which it renders latent, or retains associated with itself, as long as it remains liquid, but of which heat no evidence can be afforded by any thermoscopic test.

I believe this and similar phenomena, where heat is connected with a change of state, may be explained and distinctly comprehended without recourse to the conception of latent heat, though it requires some effort of the mind to divest itself of this idea, and to view the phenomena simply in their dynamical relations. To assist us in so viewing them, let us first parallel with purely mechanical actions certain simple effects of heat, where change of state (I mean such change as from the solid to the liquid, or liquid to the gaseous state) is not concerned. Thus, place within a receiver a bladder, and heat the air within to a higher temperature than that without it, the bladder expands; so, force the air mechanically into it by the air-pump, the bladder expands; cool the air on the outside, or remove its pressure mechanically by an exhausting pump, the bladder also expands; conversely, increase the external repellent force, either by heat or mechanical pressure, and the bladder contracts. In the mechanical effects, the force which produced the distension is derived from, and at the expense of, the mechanical power employed, as from muscular force, from gravitation, from the reacting elasticity of springs, or any similar force by which the air-pump may be worked. In the heating effects, the force is derived from the chemical action in the lamp or source of heat employed.

Let us next consider the experiment so arranged that the

force, which produces expansion in the one case, produces a correlative contraction in the other: thus, if two bladders, with a connecting neck between them, be half-filled with air, as the one is made to contract by pressure the other will dilate, and *vice versâ*; so a bladder partly filled with cold air, and contained within another filled with hot air, expands, while the space between the bladders contracts, exhibiting a mere transfer of the same amount of repulsive force, the mobility of the particles, or their mutual attraction, being the same in each body; in other words, the repulsive force acts in the direction of least resistance until equilibrium is produced; it then becomes a static or balanced, instead of a dynamic or motive force.

Let us now consider the case where a solid is to be changed to a liquid, or a liquid to a gas; here a much greater amount of heat or repulsive force is required, on account of the cohesion of the particles to be separated. In order to separate the particles of the solid, precisely as much force must be parted with by the warmer liquid body as keeps an equal quantity of it in its liquid state; it is, indeed, only with a more striking line of demarcation, the case of the hot and cold bladder—a part of the repellent power of the hot particles is transferred to the cold particles, and separates them in their turn, but the antagonist force of cohesion or aggregation necessary to be overcome, being in this case much stronger, requires and exhausts an exactly proportionate amount of repellent force mechanically to overcome it; hence the different effect on a body such as the common thermometer, the expanding liquid of which does not undergo a similar change of state. Thus, in the example above given, of the mixture of cold with hot water, the hot and cold water and the mercury of the thermometer being all in a liquid state before, and remaining so after contact, the resulting temperature is an exact mean; the hot water contracts to a certain extent, the cold water expands to the same extent, and the thermometer either sinks or rises the same number of degrees, accordingly as it had been previously immersed in the cold or in the hot solution, its mercury gaining or losing an equivalent of repellent force. In the second instance, viz. the mixture of ice with hot

water, the substance we use as an indicator, *i.e.* mercury, does not undergo the same physical change as do those whose relations of volume we are examining. The force—viewing heat simply as mechanical force—which is employed in loosening or tearing asunder the particles of the solid, ice, is abstracted from the liquid, water, and from the liquid mercury of the thermometer, and in proportion as this force meets with a greater resistance in separating the particles of a solid than of a liquid, so the bodies which yield the force suffer proportionately a greater contraction.

If we compare the action of heat on the two substances, water and mercury alone, and throw out of our consideration the ice, we shall be able to apply the same view: thus, if a given source of heat be applied to water containing a mercurial thermometer, both the water and mercury gradually expand, but in different degrees; at a certain point the attractive force of the molecules of the water is so far overcome that the water becomes vapour. At this point, the heat or force, meeting with much less resistance from the attraction of the particles of steam than from those of the mercury, expends itself upon the former; the mercury does not further expand, or expands in an infinitesimally small degree, and the steam expands greatly. As soon as this arrives at a point where circumambient pressure causes its resistance to further expansion to be equal to the molecular resistance to expansion in the mercury of the thermometer, the latter again rises, and so both go on expanding in an inverse ratio to the resisting force. If the circumambient pressure be increased, as by confining the water at the commencement of the experiment within a less expansible body than itself, such as a metallic chamber, then the mercury of the thermometer continues to rise; and if the experiment were continued, the water being confined and not the mercury, until we have arrived at a degree of repulsive force which is able to overcome the cohesive power of the mercury, so that this expands into vapour, then we get the converse effect; the force expends itself upon the mercury, which expands indefinitely, as the water did in the first case, and the water does not expand at all.

Another very usual mode of regarding the subject may embarrass at first sight, but a little consideration will show that it is explicable by the same doctrine. Water which has ice floating in it will give, when measured by the thermometer, the same temperature as the ice; *i.e.* both the water and ice contract the mercury of the thermometer to the point conventionally marked as 32°. It may be said, how is this reconcilable with the dynamical doctrine, for according to that the solid should take from the mercury of the thermometer more repulsive power than the liquid; consequently, the ice should contract the mercury more than the water?

My answer is, that in the proposition as thus stated, the quantities of the water, ice, and mercury are not taken into consideration, and hence a necessary dynamical element is neglected: if the element of quantity be included, this objection will not apply. Let the thermometer, for instance, contain 13½ oz. of mercury, and stand at 100°; if placed in contact with an unlimited quantity of ice at 32°, the mercury will sink to 32°. If the same thermometer be immersed in an unlimited quantity of water at 32°, the mercury sinks also to 32°; not absolutely, perhaps, because, however great the quantity of water or ice, it will be somewhat raised in temperature by the warmer mercury. This elevation of temperature above 32° will be smaller in proportion as the quantity of water or ice is larger than the quantity of mercury; and, as we know of no intermediate state between ice and water, the contact of a thermometer at a temperature above the freezing point with any quantity of ice exactly at the freezing point would, theoretically speaking, liquefy the whole, provided it had sufficient time; for as every portion of that ice would in time have its temperature raised by the contact of the warmer body, and as any elevation of temperature above the freezing point liquefies ice, every portion should be liquefied. Practically speaking, however, in both cases, that of the water and of the ice, when the quantity is indefinitely great the thermometer falls to 32°.

Now place the same thermometer at 100° successively in one oz. of water at 32°, and in one of ice at 32°; we shall find in the former case it will be lowered only to 54°, and in

the latter to 32°; apply to this the doctrine of repulsive force, and we get a satisfactory explanation

In the first case, the quantity both of ice and water being indefinitely great in respect to the mercury, each reduces it to its own temperature, viz. 32°, and the ice cannot reduce the mercury below 32°, because the latter would receive back repulsive power from the newly-formed water, and this would become ice; in the second case, where the quantities are limited, the mercury does lose more repulsive power by the ice than by the water, and the observations made in reference to the first illustration apply.

The above doctrine is beautifully instanced in the experiment of Thilorier, by which carbonic acid is solidified. Carbonic acid gas, retained in a strong vessel under great pressure, is allowed to escape from a small orifice; the sudden expansion requires so great a supply of force, that in furnishing the demands of the expanding gas certain other portions of the gas contract to such an extent as to solidify: thus, we have reciprocal expansion and contraction going on in one and the same substance, the time being too limited for the whole to assume a uniform temperature, or in other words a uniform extent of expansion.

It has been observed with reference to heat thus viewed, that it would be as correct to say, that heat is absorbed, or cold produced by motion, as that heat is produced by it. This difficulty ceases when the mind has been accustomed to regard heat and cold as themselves motion, *i.e.* as correlative expansions and contractions, each being evidenced by relation, and being inconceivable as an abstraction.

For instance, if the piston of an air-pump be drawn down by a weight, cold is produced in the receiver. It may be here said that a mechanical force, and the motion consequent upon it, produces cold; but heat is produced on the opposite side of the piston, if a receiver be adapted so as to retain the compressed air. Assuming them to be equivalent to each other, the force of the falling weight would be expressed by the heat of friction of the piston against its tube, and by the tension or power of reaction of the compressed against the dilated air. If the heat due to compression be made to perform

mechanical work, it would *pro tanto* be consumed, and could not restore the temperature to the dilated air; but if it perform no work, no heat is lost. Mr. Joule has experimentally proved this proposition.

In commencing the subject of heat, I asked my reader to put out of consideration the sensations which heat produces in our own bodies: I did this because these sensations are likely to deceive, and have deceived many as to the nature of heat. These sensations are themselves occasioned by similar expansions to those which we have been considering; the liquids of the body are expanded, *i.e.* rendered less viscid by heat, and from their more ready flow we obtain the sensation of agreeable warmth. By a greater degree of heat, their expansion becomes too great, giving rise to a sense of pain, and if pushed to extremity, as with the heat which produces a burn, the liquids of the body are dissipated in vapour, and an injury or destruction of the organic structure takes place. A similar though converse effect may be produced by intense cold; the application of frozen mercury to the animal body produces a burn similar to that produced by great heat, and accompanied with a similar sensation.

Doubtless other actions than those above-mentioned interfere in producing the sensations of heat and cold; but I think it will be seen that these will not affect the arguments as to the nature of heat. The phenomenal effects will be found unaltered: heat will still be found to be expansion, cold to be contraction; and the expansion and contraction are, as with the two bladders of air, correlative—*i.e.* we cannot expand one body, A, without contracting some other body, B; we cannot contract A without expanding B, assuming that we view the bodies with relation to heat alone, and suppose no other force to be manifested.

I have said that there are a few exceptions as to heat being always manifested by an expansion of matter. One class of these exceptions is only apparent: moist clay, animal or vegetable fibre, and other substances of a mixed nature, which contain matter of diverse character, some of which is more and some less volatile, *i.e.* expansible, are contracted on the application of heat; this arises from the more volatile matter

being dissipated in the form of vapour or gas; and the interstices of the less volatile being thus emptied, the latter contracts by its own cohesive attraction, giving thus a *primâ facie* appearance of contraction by heat. The pyrometer of Wedgwood is explicable on this principle.

The second class of exceptions, though much more limited in extent, is less easily explained. Water, fused bismuth, and probably some other substances (though the fact as to them is not clearly established), expand as they approach very near to the freezing or solidifying point. The most probable explanation of these exceptions is, that at the point of maximum density the molecules of these bodies assume a polar or crystalline condition; that by the particles being thus arranged in linear directions like *chevaux de frise*, interstitial spaces are left, containing matter of less density, so that the specific density of the whole mass is diminished.

Some experiments of Dr. Tyndall on the physical properties of ice seem to favour this view. When a sunbeam, concentrated by a lens, is allowed to fall on a piece of apparently homogeneous ice, the path of the rays is instantly studded with numerous luminous spots like minute air-bubbles, and the planes of freezing are made manifest by these and by small fissures. Stars or flower-like figures of six petals appear parallel to the planes of freezing, and seemingly spreading out from a central bubble. These flowers are formed of water. When the ice is melted in warm water no air is given off from the bubbles, so they seem to be vacuous; it is, however, possible that extremely minute particles of air sufficient to form foci for the melting points of ice might be dissolved by the water as soon as it surrounds them. Be this as it may, the existence of these points throughout the ice, where it gives way to the heat of the solar beam, if it does not prove actual vacuous or aeriform spaces to exist in ice, proves that it is not homogeneous, that its structure is probably definitely crystalline, and that the matter composing it is in different degrees of aggregation, so that its mean specific gravity might well be less than that of water.

We cannot examine piecemeal the ultimate structure of

matter, but in addition to the fact that the bodies which evince this peculiarity are bodies which, when solidified, exhibit a very marked crystalline character, there are experiments which show that water between the point of maximum density and its point of solidification polarises light circularly; showing, if these experiments be correct, a structural alteration in water, and one analogous to that possessed by certain crystalline solids, and to that possessed by water itself, where it is forcibly made to assume a polarised condition by the influence of magnetism.

The accuracy of these results has, however, been doubted, and the experiments have not succeeded when repeated by very experienced hands. Whether this be so or not, and whether the above explanation of the exception to the otherwise invariable effect of expansion by heat be or be not regarded as admissible, must be left to the judgment and experience of each individual who thinks upon the subject; at all events, no theory of heat yet proposed removes the difficulty, and therefore it equally opposes every other view of the phenomena of heat, as it does that which I have here considered, and which regards heat as communicable expansive force.

As certain bodies expand in freezing, and indeed in some cases before they arrive at the temperature at which they solidify, we get the apparent anomaly that the motion or mechanical force generated by heat or change of temperature is reversed in direction when we arrive at the point of change from the solid to the liquid state. Thus, a piece of ice, at the temperature of Zero, Fahrenheit, would expand by heat, and produce a mechanical force by such expansion until it arrives at 32°; but then by an increment of heat it contracts; and if the first expansion had moved a piston upwards, the subsequent contraction would bring it back to a certain extent, or move it downwards, an apparent negation of the force of heat.

Again, with water above 39°, *i.e.* above its point of maximum density, a progressive increment of cold or decrement of heat would produce contraction to a certain point, and then expansion or a mechanical force in an opposite direction.

Thus not only heat or the expansive force given to other bodies by a body cooling would be given out by water freezing, but also the force due to the expansion in the body itself, and force would thus seem to be got out of nothing; but if water in a confined space be gradually cooled, the expansion attendant on its cooling as it approaches the freezing point would occasion pressure amongst its particles, and thence tend to antagonise the force of dilatation produced in them by cooling, or to resist their tendency to freeze; or in other words the pressure would tend to liquefaction, and conversely to the usual effect of pressure, produce cold instead of heat, and thus neutralise some of the heat yielded by the cooling body. Hence we find that it requires a lower temperature to freeze water under pressure than when exempt from it, or that the freezing point is lowered as the pressure increases for bodies which expand in freezing—an effect first predicted by Mr. J. Thomson, and experimentally verified by Sir W. Thomson; while, as shown by M. Bunsen, the converse effect takes place with bodies which contract in freezing; here the pressure co-operates with the effects of cold, both tending to approximate the particles, and such substances solidify at a higher temperature in proportion as the pressure is greater; so that we might expect a body of this class, which under the ordinary pressure of the air is at a temperature just above its freezing point, to solidify by being submitted to pressure alone, the temperature being kept constant.

A similar class of exception to the general effect of heat in expanding bodies is presented by vulcanised caoutchouc. This has been observed by Mr. Gough, and, indeed, was pointed out to me many years ago by Mr. Brockedon, to be heated when stretched, and cooled when unstretched.

Mr. Joule finds that its specific gravity is less when stretched than when unstretched, and that when heated in its stretched state it shortens, presenting in this particular condition a similar series of relations to those which are presented by water near or at its freezing point.

With the exception of this class of phenomena, which offer difficulties to any theory which has been proposed, the general phenomena of heat may, I believe, be explained upon a purely

dynamical view, and more satisfactorily than by having recourse to the hypothesis of latent matter. Many, however, of the phenomena of heat are involved in much mystery, particularly those connected with specific heat or that relative proportion of heat which equal weights of different bodies require to raise them from a given temperature to another given temperature, which appears to depend in some way hitherto inexplicable upon the molecular constitution of different bodies.

The view of heat which I have taken, viz. to regard it simply as a communicable molecular repulsive force, is supported by many of the phenomena to which the term specific or relative heat is applied; for example, bodies as they increase in temperature increase in specific heat. The ratio of this increase in specific heat is greater with solids than with liquids, although the latter are more dilatable; an effect probably depending upon the commencement of fusion. Again, those metals whose rate of expansion increases most rapidly when they are heated, increase most in specific heat; and their specific heat is reduced by percussion, which, by approximating their particles, makes them specifically more dense. When, however, we examine substances of very different physical characters, we find that their specific heats have no relation to their density or rate of expansion by heat; their differences of specific heat must depend upon their intimate molecular constitution in a manner accounted for (as far as I am aware) by no theory of heat hitherto proposed.

In the greater number, probably in all solids and liquids, the expansion by heat is relatively greater as the temperature is higher; or, preserving the view of expansion and contraction, if two equal portions of the same substance be juxtaposed at different temperatures, the hotter portion will contract a little more than the colder will expand; from this fact, viz. that the coefficient of expansion increases in a given body with the temperature, and from other considerations, Dr. Wood has argued, with much apparent reason, that the nearer the particles of bodies are to each other, the less they require to move to produce a given expansion or contraction in those of

another body. His mode of reasoning, if I rightly conceive it, may be concisely put as follows :—

As bodies contract by cold, it is clear that, in a given body, the lower the temperature the nearer are the particles; and, as the coefficient of expansion increases with the temperature, the lower the temperature of the substance be, the less the particles require to move, or approach to or recede from each other, so as to compensate the correlative recession or approach of the particles in a hotter portion of the same substance, that is, in another portion of the same substance in which the particles are more distant from each other. The amount of approximation or recession of the particles of a body, in other words, its change of bulk by a given change of temperature, being thus in a given substance an index of the relative proximity of its particles, may it not be so of all bodies? The proposition is very ingeniously argued by Dr. Wood, but the argument is based upon certain hypotheses as to the sizes and distances of atoms, which must be admitted as postulates by those who adopt his conclusions. Dr. Wood seeks by means of this theory to explain the heat produced by chemical combination, and I shall endeavour to give a sketch of his mode of reasoning when I arrive at that part of my subject.

Although the comparative effects of specific heat may not be satisfactorily explicable by any known theory, the absolute effect of heat upon each separate substance is simply expansion, but when bodies differing in their physical characters are used, the rate of expansion varies, if measured by the correlative contractions exhibited by the substances producing it. Though I am obliged, in order to be intelligible, to talk of heat as an entity, and of its conduction, radiation, &c., yet these expressions are, in fact, inconsistent with the dynamic theory which regards heat as motion and nothing else; thus conduction would be simply a progressive dilatation or motion of the particles of the conducting substance, radiation an undulation or motion of the particles of the medium through which the heat is said to be transmitted, &c.; and it is a strong argument in favour of this theory, that for every diver-

sity in the physical character of bodies, and for every change in the structure and arrangement of particles of the same body, a change is apparent in the thermal effects. Thus gold conducts heat, or transmits the motion called heat, more readily than copper, copper than iron, iron than lead, lead than porcelain, &c.

So when the structure of a substance is not homogeneous, we have a change in the conduction of heat by different parts which is dependent upon the structure. This is beautifully shown with bodies whose structure is symmetrically arranged, as in crystals. Senarmont has shown that crystals conduct heat differently in different directions with reference to the axis of symmetry, but definitely in definite directions. His mode of experimenting is as follows :—A plate of the crystal is cut in a direction, for one set of experiments parallel, and for another at right angles to the axis ; a tube of platinum is inserted through the centre of the plate, and bent at one extremity, so as to be capable of being heated by a lamp without the heat which radiates from the lamp affecting the crystal ; the surfaces of the plate of crystal are covered with wax. When the platinum is heated, the direction of the heat conducted by the crystal is made known by the melting of the wax, and a curved line is visible at the juncture of the solid and liquid wax. This curve, with homogeneous substances, as glass or zinc, is a circle; it is also a circle on plates of calc spar cut perpendicular to the axis of symmetry ; but on plates cut parallel to the axis of symmetry, and having their plane perpendicular to one of the faces of the primitive rhombohedron, the curves are well-defined ellipses, having their longer axes in the direction of the axis of symmetry, showing that this axis is a direction of greater conductibility. From experiments of this character the inference is drawn, that 'in media constituted like crystals of the rhombohedral system, the conducting power varies in such a manner, that, supposing a centre of heat to exist within them, and the medium to be indefinitely extended in all directions, the isothermal surfaces are concentric ellipsoids of revolution round the axis of symmetry, or at least surfaces differing but little therefrom.'

Knoblauch has further shown, that radiant heat is absorbed

in different degrees, according as its direction is parallel or perpendicular to the axis of a crystal.

If we select a substance of a different but also of a definite structure, such as wood, we find that heat progresses through it with more or less rapidity, according to its direction with reference to the fibre of the wood : thus Decandolle and De la Rive found that the conduction was better in a direction parallel to the fibre than in one transverse to it ; and Dr. Tyndall has added the fact, that the conduction is better in a direction transverse to the fibre and layers of the wood than when transverse to the fibre but parallel to the layers, though in both these directions the conduction is inferior to that following the direction of the fibre. Thus, in the three possible directions in which the structure of wood may be contemplated, we have three different degrees of progression for heat.

In the above examples we see, as we shall see farther on with reference to all the so-called imponderables, that the phenomena depend upon the molecular structure of the matter affected ; and although these facts are not absolutely inconsistent with the theory which supposes them to be fluids or entities, it will, I think, be found to be far more consistent with that which views them as motion. Heat, which we are at present considering, cannot be insulated : we cannot remove the heat from a substance and retain it as heat ; we can only transmit it to another substance, either as heat or as some other mode of force. We only know certain changes of matter, for which changes heat is a generic name ; the *thing* heat is unknown.

Heat having been shown to be a force capable of producing *motion*, and motion to be capable of producing the other modes of force, it necessarily follows that heat is capable, mediately, of producing them : I will, therefore, content myself with enquiring how far heat is capable of immediately producing the other modes of force. It will immediately produce *electricity*, as shown in the beautiful experiments of Seebeck, one of which I have already cited, which experiments proved, that when dissimilar metals are made to touch, or are soldered together, and heated at the point of contact, a current of electricity flows through the metals having a definite direction

according to the metals employed, which current continues as long as an increasing temperature is gradually pervading the metals, ceases when the temperature is stationary, and flows in the contrary direction with the decrement of temperature.

Another class of phenomena which have been generally attributed to the effects of radiant heat, and to which, from this belief, the term thermography has been applied, may also, in their turn, be made to exhibit electrical effects—effects here of Franklinic or static electricity, Seebeck's experiments showing effects of voltaic or dynamic electricity.

If polished discs of dissimilar metals—say, zinc and copper —be brought into close proximity, and kept there for some time, and either of them has irregularities upon its surface, a superficial outline of these irregularities is traceable upon the other disc, and *vice versâ*. Many theories have been framed to account for this phenomenon, but whether it be due or not to thermic radiations, the relative temperature of the discs, their relative capacities and conducting and radiating powers for heat, undoubtedly influence the phenomena.

Now, if two such discs in close proximity be connected with a delicate electroscope, and then suddenly separated, the electroscope is affected, showing that the reciprocal radiation from surface to surface has produced electrical force. I cite this experiment in treating of heat as an initial force, because at present the probabilities are in favour of thermic radiation producing the phenomenon. The origin of these so-called thermographic effects is, however, a question open to much doubt, and needs much further experiment. When I first published the experiment which showed that the mere approximation of discs of dissimilar metals would give rise to electrical effects, I mentioned that I considered the fact of the superficial change upon the surface of metals in proximity, and, *à fortiori*, in contact, would explain the development of electricity in Volta's original contact experiment, without having recourse to the contact theory, *i.e.* a theory which supposes a force to be produced by mere contact of dissimilar metals without any molecular or chemical change. I have seen nothing to alter this view. Mr. Gassiot has repeated and verified my experiment with more delicate apparatus and

under more unexceptionable circumstances; and without saying that radiant heat is the initial force in this case, we have evidence, by the superficial change which takes place in bodies closely approximated, that some molecular change is taking place, some force is called into action by their proximity, which produces changes in matter as it expends, or rather transmits itself; and, therefore, is not a force without molecular change, as the supposed contact force would be. The force in this, as in all other cases, is not created, but developed by the action of matter on matter, and not annihilated, as it is shown by this experiment to be convertible into another mode of force.

To say that heat will produce *light*, is to assert a fact apparently familiar to everyone, but there may be some reason to doubt whether the expression to produce light is correct in this particular application; the relation between heat and light is not analogous to the correlation between these and the other four affections of matter; heat and light appear to be rather modifications of the same force than distinct forces mutually dependent. The modes of action of radiant heat and of light are so similar, both being subject to the same laws of reflection, refraction, double refraction, and polarisation, that their difference appears to exist more in the manner in which they affect our senses than in their modes of physical action.

The experiments of Melloni, which have mainly contributed to demonstrate the analogies of heat and light, afford a beautiful instance of the assistance which the progress of one branch of physical science renders to that of another. The discoveries of Oersted and Seebeck led to the construction of an instrument for measuring temperature incomparably more delicate than any previously known. To distinguish it from the ordinary thermometer, this instrument is called the *thermo-multiplier*. It consists of a series of small bars of bismuth and antimony, forming one zigzag chain of alternations arranged parallel to each other, and forming together a cylinder or prism; so that the points of junction, which are soldered, shall be all exposed at the bases of the cylinder: the two extremities of this series are united to a galvanometer—that is, a coil of wire surrounding a freely-suspended mag-

netic needle, the direction of which is parallel to the convolutions of the wire. When radiant heat impinges upon the soldered ends of the multiplier, a thermo-electric current is induced in each pair; and, as all these currents tend to circulate in the same direction, the energy of the whole is increased by the co-operating forces: this current, traversing the helix of the galvanometer, deflects the needle from parallelism by virtue of the electro-magnetic tangential force, and the degree of this deflection serves as the index of the temperature.

Bodies examined by these means show a remarkable difference between their transcalescence or power of transmitting heat, and their transparency: thus, perfectly transparent alum arrests more heat than quartz so dark-coloured as to be opaque; again alum coupled with green glass Melloni found was capable of transmitting a beam of brilliant light, while, with the most delicate thermoscope, he could detect no indications of transmitted heat: on the other hand, rock salt, the most transcalescent body known, may be covered with soot until perfectly opaque, and yet be found capable of transmitting a considerable quantity of heat. Radiant heat, when transmitted through a prism of rock-salt, is found to be unequally refracted, as is the case with light; and the rays of heat thus elongated into what is, for the sake of analogy, called a spectrum, are found to possess similar properties to the primary or coloured rays of light. Thus rock-salt is to heat what colourless glass is to light: it transmits heat of all degrees of refrangibility; alum is to heat as red glass to light: it transmits the least, and stops the most refrangible rays; and rock-salt covered with soot represents blue glass, transmitting the most, and stopping the least refrangible rays.

Certain bodies, again, reflect heat of different refrangibility; thus, paper, snow, and lime, although perfectly white—that is, reflecting light of all degrees of refrangibility, reflect respectively heat only of certain degrees; while metals, which are coloured bodies—that is, bodies which reflect light only of certain degrees of refrangibility—reflect heat of all degrees. Radiant heat incident upon substances which doubly refract light is doubly refracted; and the emergent rays are polarised

in planes at right angles to each other, as is the case with light.

The relation of radiation to absorption also holds good with light as with heat: with the latter it has been long known that the radiating power of different substances is directly proportional to their absorptive and inverse to their reflective power; or rather, that the sum of the heat radiated and reflected is a constant quantity. Further, it has been shown by Mr. Balfour Stewart, that the absorption of heat is proportional to its radiation, both as to quality and quantity.

Light presents us with similar relations. Coloured glass, when heated so as to be luminous, emits the same light which at ordinary temperatures it absorbs: thus red glass gives out or radiates a greenish light, and green glass a red tint.

The flame of substances containing sodium yields a yellow light of such purity that other colours exposed to it appear black—a phenomenon shown by the familiar experiment of exposing a picture of bright colours, other than yellow, to the flame of spirits of wine with which common salt is mixed: the picture loses its colours, and appears to be black and white. When the prismatic spectrum of such a flame is examined, it is found to exhibit two bright yellow lines at a certain fixed position. If a source of light be employed which gives no lines in its spectrum, and the light of this incandescent substance be made to pass through the sodium flame, two dark lines will appear in the spectrum precisely coincident in position with the yellow lines which were given by the sodium flame itself. The same relation of absorption to radiation is therefore shown here: the substance absorbs that light which it yields when it is itself the source of light, The same is true of other substances, the spectra of which exhibit respectively lines of peculiar colour and position. Now, the solar prismatic spectrum is traversed by a great number of dark lines; and Kirchhoff has deduced from considerations such as those which I have shortly stated, that these dark lines in the solar spectrum are due to metals or other substances existing in an atmosphere around the sun which absorb the light from a central incandescent nucleus, each metal absorbing that

light which would appear as a bright line or lines in the spectrum produced by its own light.

By comparing the positions of the bright lines in the spectra of various metals with those of the dark lines in the solar spectrum, several of them are found to be in identically the same place: hence it is inferred, and the inference seems reasonable, that the metals which show luminous lines in their spectra, identical in position with dark lines in the solar spectrum, exist in the sun, and are diffused in a gaseous state in its atmosphere. It does not seem to me necessary to this conclusion to assume that the sun is a solid mass of incandescent matter: it may well be that what we term the photosphere or luminous envelope of the sun has surrounding it a more diffuse atmosphere containing vaporised metals, and that the mass of the sun itself may be in a different state, and not necessarily in an incandescent temperature; indeed, the protuberances and red light seen at the period of total eclipses afford some evidence of an atmosphere exterior to the photosphere. It would, however, be out of place here to speculate on these subjects: the point which concerns us is the analogy of heat and light, which these discoveries illustrate. Kirchhoff has carried the analogy farther, by showing that a plate of tourmaline absorbs the polarised ray which when heated it radiates. Thus, the phenomena of light are imitated closely by those of radiant heat; and the same theory which is considered most plausibly to account for the phenomena of the one, will necessarily be applied to the other agent, and in each case molecular change is accompanied by a change in the phenomenal effects.

In certain cases heat appears to become partially converted into light, by changing the matter affected by heat: thus gas may be heated to a very high point without producing light, or producing it to a very slight degree; but the introduction of solid matter—for instance, the metal platinum into the highly-heated gas—instantly exhibits light. Whether the heat is converted into light, or whether it is concentrated and increased in intensity by the solid matter so as to become visible, may be open to some doubt: the fact of solid matter when ignited by the oxyhydrogen jet, decomposing water,

as will be presently explained, would seem to indicate that the heat was rendered more intense by condensation in the solid matter, as water is in this case decomposed by a heated body, which body has itself been heated by the combining elements of water. The apparent effect, however, of the introduction of solid incombustible matter into heated gas, is a conversion of heat into light.

Dr. Tyndall, by passing light from the voltaic arc through solutions of iodine, separates the invisible rays of heat from the luminous rays, and then reproduces light by receiving the former on platinum foil.

If we concentrate into a focus by a large lens a dim light, we increase the intensity of the light. Now, if a heated body be taken, which, to the unassisted eye, has just ceased to be visible, it seems probable that by collecting and condensing by a lens the different rays which have so ceased to be visible, light would reappear at the focus. The experiment is, for reasons obvious to those acquainted with optics, a difficult one, and to be conclusive, should be made on a large scale, and with a very perfect lens of large diameter and short focus. I have obtained an approximation to the result in the following manner:—In a dark room a platinum wire is brought just to the point of visible ignition by a constant voltaic battery; it is then viewed, at a short distance, through an opera-glass of large aperture applied to one eye, the other being kept open. The wire will be distinctly visible to that eye which regards it through the opera-glass, and at the same time totally invisible to the other and naked eye. It may be said with some justice that such experiments prove little more than the fact already known, viz. that by increasing the intensity of heat, light is produced; they however exhibit this effect in a more striking form, as bearing on the relations of heat and light.

With regard to *chemical affinity* and *magnetism*, perhaps the only method by which in strictness the force of heat may be said to produce them is through the medium of electricity, the thermo-electrical current, produced, as before described, by heating dissimilar metals, being capable of deflecting the magnet, of magnetising iron, and exhibiting the other mag-

netic effects, and also of forming and decomposing chemical compounds, and in some proportion to the progression of heat: this has not, indeed, as yet been proved to bear a measurable quantitative relation to the other forces thus produced by it, because so little of the heat is utilised or converted into electricity, much being dissipated, without change in the form of heat.

Heat, however, directly affects and modifies both the magnet and chemical compounds; the union of certain chemical substances is induced by heat, as, for instance, the formation of water by the union of oxygen and hydrogen gases: in other cases this union is facilitated by heat, and in many instances, as in ammonia and its salts, it is weakened or antagonised. In many of these cases, however, the force of heat seems more a determining than a producing influence; yet to be this, it must have an immediate relation with the force whose reaction it determines: thus, although gunpowder, touched with an ignited wire, subsequently carries on its own combustion or chemical combination, independently of the original source of heat, yet the chemical affinities of the first portion touched must be exalted by, and at the cost of, the heat of the wire; for to disturb even an unstable equilibrium requires a force in direct relation with those which maintain the equilibrium.

Shortly after the first edition of this essay was published, I communicated to the Royal Society some experiments by which an important exception to the general effect of heat on chemical affinity is removed, and the results of which induce a hope that a generalised relation will ultimately be established between heat, chemical affinity, and physical attraction. I find that if a substance capable of supporting intense heat and incapable of being acted upon by water or either of its elements—such a substance, for instance, as platinum, or iridium—be raised to a high point of ignition and then immersed in water, bubbles of permanent gas ascend from it, which on examination are found to consist of mixed oxygen and hydrogen in the proportions in which they form water. The temperature at which this is effected is, according to Dr. Robinson, who has since written a valuable paper on the subject, = 2386°. Now, when mixed oxygen and hydrogen are exposed to a temperature of about 800°, they combine

and form water; heat therefore appears to act differently upon these elements according to its intensity, in one case producing composition, in the other decomposition. No satisfactory means of reconciling this apparent anomaly have been pointed out : the best approximation to a hypothesis which I can frame is by assuming that the constituent molecules of water are, below a certain temperature, in a state of stable equilibrium ; that the molecules of mixed or oxyhydrogen gas are, above a certain temperature, also in a state of stable equilibrium, but of an opposite character ; while below this latter temperature the molecules of mixed gas are in a state of unstable equilibrium, somewhat similar to that of the fulminates or similar bodies, in which a slight derangement subverts the nicely-balanced forces.

If, for instance, we suppose four molecules, A, B, C, D, to be in a balanced state of equilibrium between attracting and repelling forces, the application of a repulsive force between B and C, though it may still farther separate B and C, will approximate B to A and C to D, and may bring them respectively within the range of attractive force ; or, supposing the repulsive force to be in the centre of an indefinite sphere of particles, all these, excepting those immediately acted on by the force, will be approximated, and having from attraction assumed a state of stable equilibrium, they will retain this, because the repulsive force divided by the mass is not capable of overcoming it. But if the repulsive force be increased in quantity and of sufficient intensity, then the attractive force of all the molecules may be overcome, and decomposition ensue. Thus, water or steam below a certain temperature, and mixed gas above a certain temperature, may be supposed to be in the state of stable equilibrium, whilst between these limiting temperatures, the equilibrium of oxyhydrogen gas is unstable.

This, it must be confessed, is but a crude mode of explaining the phenomena, and requires the assumption, that chemical attraction or affinity, as it is awkwardly termed, has similar attributes to physical attraction. Chemical affinity holds good between the particles of matter when it is in a gaseous state, and though gases do not give evidence of the physical

attraction of cohesion or aggregation, yet both gases and solids expand or contract according to the inverse contraction or expansion of other neighbouring bodies, and so far resemble each other in their relations to heat and cold. The extent to which such expansion or contraction can be carried, seems to be limited only by the correlative state of other bodies; these, again, by others, and so on, as far as we may judge, throughout the universe.

Adopting the explanation above given of the decomposition of water by heat, heat would have the same relation to chemical affinity as it has to physical attraction; its immediate tendency is antagonistic to both, and it is only by a secondary action that chemical affinity is apparently promoted by heat. This hypothesis would account for heat promoting changes of the equilibrium of chemical affinity among mixed compound substances, by decomposing certain compounds and separating elementary constituents whose affinity is greater, when they are brought within the sphere of attraction for the substance with which they are mixed, than for those with which they were originally chemically united: thus an intense heat being applied to a mixture of chlorine and the vapour of water, occasions the production of muriatic acid, liberating oxygen.

Carrying out this view, it would appear that a sufficient intensity of heat might yield indefinite powers of decomposition; and there seems some probability of bodies now supposed to be elementary, being decomposed or resolved into further elements by the application of heat of sufficient intensity; or, reasoning conversely, it may fairly be anticipated that bodies, which will not enter into combination at a certain temperature, will enter into combination if their temperature be lowered, and that thus new compounds may be formed by a proper disposition of their constituents when exposed to an extremely low temperature, and the more so if compression be also employed.

The term 'dissociation' has been applied by M. St. Clair Deville to my experiment of the separation of oxygen and hydrogen by heat, and to other cases, in which heat separates the constituents of a substance without any of them combining with another body, thus contradistinguishing these effects from

those of ordinary chemical decomposition where the affinity of other substances is brought into play.

In considering the effect of heat as a mechanical force, it would be expected, *à priori*, and independently of any theory of heat which may be adopted, that a given amount of heat acting on a given material must produce a given amount of motive power; and the next question which occurs to the mind is, whether the same amount of heat would produce the same amount of mechanical power, whatever be the material acted on or affected by the heat. I will endeavour to reason this out on the view of heat which I have advocated. Heat has been considered in this essay as itself motion or mechanical power, and quantity of heat as measured by motion. Thus, if by a given contraction of a body (say hot mercury) air within a cylinder having a movable piston be expanded, the piston moves, and in this case the expansion or motion of the material (say iron) of the cylinder itself and of the air surrounding it is commonly neglected. As the air dilates it becomes colder; in other words, by undergoing expansion itself it loses its power of making neighbouring bodies expand; but if the piston be forcibly kept down, the expansive power due to the mercury continues to communicate itself to the iron and to the surrounding air, which become hotter than they would if the piston had given way.

Now, in the above case, if the air be confined and its volume unchanged, will the expansion of the iron, assuming that it can be utilised, produce an exactly equivalent mechanical effect to that which the expansion of the air would produce if the heat be entirely confined to it?

Assuming that (with the exception of bodies which expand in freezing, where, through a limited range of temperature, the converse effects obtain) whenever a body is compressed it is heated, *i.e.* it expands neighbouring substances; whenever it is dilated or increased in volume it is cooled, *i.e.* it contracts neighbouring substances—the conclusion appears to me inevitable that the mechanical power produced by heat will be definite, or the same for a given amount and intensity of heat, whatever be the substance acted on.

Thus, let A be a definite source of heat, say a pound of

mercury at the temperature of 400° ; let B be another equal and similar source of heat : suppose A be employed to raise a piston by dilating the air beneath it, and B to raise another piston by the dilatation of the vapour of water. Imagine the pistons attached to a beam, so that they oppose each other's action, and thus represent a sort of calorific balance. If A being applied to air could conquer B, which is applied to water, it would depress or throw back the piston of the latter, and, by compressing the vapour, occasion an increase of temperature ; this, in its turn, would raise the temperature of the source of heat, so that we should have the anomaly that a pound of mercury at 400° could heat another pound of mercury at 400° to 401°, or to some point higher than its original temperature, and this without any adventitious aid : it will be obvious that this is contradictory to the whole range of our experience.

The above experiment is ideal, and stated for the object of giving a more precise form to the reasoning ; to bring the idea more prominently into relief, all statements as to quantities, specific heats, &c., so as to yield comparative results for given materials, are omitted. The argument may be thus stated in another form, viz. that by no mechanical appliance or difference of material acted on can a given source of heat be made to produce more heat than it originally possessed ; and that, if all be converted into mechanical power, an excess cannot be supposed, for that could be converted into a surplus of heat, and be a creation of force ; and a deficit cannot be supposed, for that would be annihilation of force. I cannot, however, see how the theoretical conception could be verified by experiment ; the enormous weights and the complex mechanical contrivances requisite to give the measure of power yielded by matter in its less dilatable forms, would be far beyond our present experimental resources. It would also be difficult to prevent the interference of molecular forces, the overcoming of which expends a part of the mechanical power generated, but which could hardly be made to appear in the result. We could not, for instance, practically realise the above conception by the construction of a machine which should act by the expansion and contraction of a bar of

iron, and produce a power equal to that of a steam engine supplied with an equal quantity of heat.

Carnot, who wrote in 1824 an essay on the motive power of heat, regarded the mechanical power produced by heat as resulting from a transfer of heat from one point to another, without any ultimate loss of heat. Thus, in the action of an ordinary steam-engine, the heat from the furnace having expanded the water of the boiler and raised the piston, a mechanical motion is produced; but this cannot be continued without the removal of the heat, or the contraction of the expanded water. This is done by the condenser, and the piston descends. But then we have apparently transferred the heat from the furnace to the condenser, and in the transfer effected mechanical motion.

Should the mechanical motion produced by heat be considered as the effect of a simple transference of heat from one point to another, or as the result of a conversion of heat into the mechanical force of which this motion is the evidence? This question leads to the following: does the heat which generates the mechanical power return to the thermal machine as heat, or is it conveyed away by the work performed?

If a definite quantity of air be heated it is expanded, and by its expansion it cools or loses some of its power of communicating heat to neighbouring bodies. That which we should have called heat if the expansion of the air had been prevented, we call mechanical effect, or may regard as converted into mechanical effect ceasing to be heat; but, throwing out of the question nervous sensation, this expansion or mechanical effect is all the evidence we have of heat, for if the air is allowed to expand freely, this expansion becomes the index of the heat; if the air be confined, the expansion of the matter of the vessel confining it, or of the mercury of a thermometer in contact with it, &c., are the indices of the heat.

If, again, the air which has been expanded be, by mechanical pressure or by other means, restored to its original bulk, it is capable of heating or expanding other substances to a degree to which it would not be equal, if it had remained in its expanded state. To produce continuous motion, or the

up-and-down stroke of a piston, we must, as I have already stated, heat and cool, just as with a magnetic machine we must magnetise and demagnetise in order to produce a continuous mechanical effect; and although, from the impossibility of insulating heat, some heat is apparently lost in the process, the result may be said to be effected by the transfer of heat from the hot to the cold body, from the furnace to the condenser. But we may equally well say that the heat has been converted into mechanical force, and the mechanical force back into heat; the effects are always correlative, as are the mechanical effects of an air-pump, with which, as we dilate the air on one side, we condense it on the other; and as we cannot dilate without the reciprocal condensation, so we cannot heat without the reciprocal cooling, or *vice versâ*.

Hitherto the resistances of the piston or of any superimposed weight have been thrown out of consideration, or, what amounts to the same thing, it has been assumed that the weight raised by the piston has descended with it. The heat has not merely been employed in dilating the air or vapour, but in raising the piston with its weight If, as the vapour is cooled, the weight be permitted to descend, its mechanical force restores the heat lost by the dilatation; but in this case no part of the power has been abstracted so as to be employed for any practical purpose; this question then follows, what takes place with regard to the initial heat, if, after the ascent of the piston, the weight be removed so as not to help the piston in its descent, but to fall upon a lever or produce some extraneous mechanical effect?

To answer this question, let us suppose a weight to rest on a piston which confines air at a definite temperature, say for example 50°, in a cylinder, the whole being assumed to be absolutely non-conducting for heat. A part of the heat of this confined air will be due to the pressure, since, as we have seen, compression of an elastic fluid produces heat.

Suppose, now, the confined air to be heated to 70°, the piston with its superincumbent weight will ascend, and the temperature, in consequence of the dilatation of the air, will be somewhat lowered, say to 69° (we will assume, for the sake

of simplicity, that the heat engendered by the friction of the piston compensates the force lost by friction).

The piston having reached its maximum of elevation, let a cold body or condenser take away 20° from the temperature of the confined air; the piston will now descend, and by the compression which the weight on it produces, will restore the 1° lost by dilatation, and when the piston reaches its original position the temperature of the air will be restored to 50°. Suppose this experiment repeated up to the maximum rise of the piston; but when the piston is at its full elevation, and the cold body applied, let the weight be tilted off, so as to drop upon a wheel, or be used for other mechanical purposes. The descending piston will not now reach its original point without more heat being abstracted; in consequence of the removal of the weight, there will not be the same force to restore the 1°, and the temperature will be 49°, or some fraction short of the original 50°. If this were otherwise, then, as the weight in falling may be made to produce heat by friction, we should have more heat than at first, or a creation of heat out of nothing—in other words, perpetual motion.

Let us now assume that this 20° supplied in the first instance was yielded by a body at 90°, of such size and material that its total capacity for heat is equal to that of the mass of confined air: this body would be reduced in temperature to 70°, in other words, our furnace would have lost 20° of heat. Let the cold body of the same size and material, used as a condenser, be at 30°. In the first experiment, the body at 30° would bring back the piston to its original point; but in the second experiment, or that where the weight has been removed, the body at 30° would not suffice to restore the piston: to effect this, the cold body or condenser must be at a lower temperature.

The question in Carnot's theory, which is not experimentally resolved, and which presents extreme experimental difficulty, is the following: Granted that a piston with a superimposed weight be raised by the thermic expansion of confined gas or vapour below it; if the elastic medium be restored to its original temperature by cooling, the weight in depressing the

piston will restore that portion of the heat which has been lost by the expansion, and by the mechanical effect consequent thereon; but if the weight be removed when at its maximum of elevation, and the piston be brought back to its starting point by a necessarily cooler body than could restore it if the weight were not removed, would the return of the piston now yield to the cooling body the heat which had been lost by the dilatation, or, in other words, should we in pulling the piston down by cold reacquire the same quantity of heat as would be restored by pressing it down by mechanical force? The argument from the impossibility of perpetual motion would say no, for if all the heat were restored, the mechanical effect produced by the fall of the weight, or the heating effect which might be made to result from this mechanical power, would be got from nothing.

Then follows another question, viz. whether, where an external or derived mechanical effect has been obtained, would the return of the piston, effected without the weight or external force to assist it, but solely by the colder body, give to this latter the same number of thermometric degrees as had been lost by the hot body in the first instance? Suppose, for instance, the cold body in our experiment to be at 20° instead of 30°, would this body gain 20°, and then reach the temperature of 40° when the piston is brought back, or would its temperature be higher or lower than 40°? The argument from the impossibility of perpetual motion does not apply here, for it does not necessarily follow that 20°, on the thermometric scale from 20° to 40°, represents an equal amount of force to 20° on the scale from 70° to 90°, and therefore it is quite conceivable that we may lose 20° from the furnace, and gain 20° in the condenser, and yet have obtained a certain amount of derived mechanical power. It will also follow, upon a consideration of the above imaginary experiments, that the greater the mechanical power required, the greater should be the difference between the temperature of the furnace and that of the condenser; but the exact relation in temperature between these for a given mechanical effect, has not, as far as I am aware, been satisfactorily established by experiment, though it has been shown that steam at high pressure produces, com-

paratively, a greater mechanical effect for the same number of degrees than steam at low pressure.

Carnot, assuming the number of degrees of temperature to be restored, but at a lower point of the thermometric scale, termed this the fall (*chute*) of caloric. The mechanical effect of heat, on this view, may be likened to that of a series of cascades on water-wheels. The highest cascade turns a wheel, and produces a given mechanical effect ; the water which has produced this cannot again effect it at the same level without being carried back to its original elevation, *i.e.* without an extra force being employed equivalent to, or rather a fraction more than the force of the descending water ; but though its power is spent with reference to the first wheel, the same water may, by falling over a new precipice upon a second wheel, again reproduce the same mechanical effect (strictly speaking, rather more, for it has approached the centre of gravity), and so on, until no lower fall can be attained. So with heat : it involves no necessity of assuming perpetual motion, to suppose that, after a given mechanical effect, produced by a certain loss of heat, the number of degrees lost from the original temperature may be restored to the condenser, but at a lower point of the thermometric scale.

If work has been done, *i.e.* if force has been parted with, the original temperature itself cannot be restored, but there is no *à priori* impossibility in the same number of degrees of heat as have been converted into work being conveyed to a condensing body so cold that, when it receives this heat, it will still be below the original temperature to which the work-producing heat was added.

In the theory of the steam-engine, this subject possesses a great practical interest. Watt supposed that a given weight of water required the same quantity of what is termed total heat (that is, the sensible added to the latent heat) to keep it in the state of vapour, whatever was the pressure to which it was subjected, and, consequently, however its expansive force varied. Clement Desormes was supposed to have experimentally verified this law. If this were so, vapour raising a piston with a weight attached would produce mechanical power ; and yet, the same heat existing as at first, there would

be no expenditure of the initial force; and if we suppose that the heat in the condenser was the real representative of the original heat, we should get perpetual motion. Southern supposed that the latent heat was constant, and that the heat of vapour under pressure increased as the sensible heat. M. Despretz, in 1832, made some experiments, which led him to the conclusion that the increase was not in the same ratio as the sensible heat, but that yet there was an increase; a result confirmed and verified with great accuracy by M. Regnault, in some recent and elaborate researches. What seems to have occasioned the error in Watt and Clement Desormes' experiments was, the idea involved in the term latent heat; by which, supposing the phenomenon of the disappearance of sensible heat to be due to the absorption of a material substance, that substance, 'caloric,' was thought to be restored when the vapour was condensed by water, even though the water was not subjected to pressure; but to estimate the total heat of vapour under pressure the vapour should be condensed while subjected to the same pressure as that under which it is generated, as was done in M. Despretz and M. Regnault's experiments.

M. Seguin, in 1839, controverted the position that derived power could be got by the mere transfer of heat, and by calculation from certain known data, such as the so-called law of Mariotte, viz. that the elastic force of gases and vapours increased directly with the pressure; and assuming that for vapour between 100° and 150° centigrade, each degree of elevation of temperature was produced by a thermal unit, he deduced the equivalent of mechanical work capable of being performed by a given decrement of heat; and thus concluded that, for ordinary pressures, about one gramme of water losing one degree centigrade would produce a force capable of raising a weight of 500 grammes through a space of one mètre; this estimate is a little beyond that given conversely by the experiments of Mr. Joule, already stated, in which the heat produced by a given amount of mechanical action is estimated. I am not aware that the amount of mechanical work which is produced by a given quantity of heat has been directly established by experiment, though some approximative results in parti-

cular cases have been given. Theoretically it should be the same—that is to say, if a fall of 772 lbs. through a space of one foot will raise the temperature of 1 lb. of water through one degree of Fahrenheit, then the fall in the temperature of 1 lb. of water through one degree of Fahrenheit should be able to raise 772 lbs. through a space of one foot. The calculations of M. Seguin are not far from this, but since the elaborate experiments of M. Regnault he has expressed some doubt of the correctness of his former estimate, as by these experiments it appears that, within certain limits, for elevating the temperature of compressed vapour by one degree, no more than about three-tenths of a degree of total heat is required; consequently, the equivalent multiplied in this ratio would be 1,666 grammes instead of 500. Other investigators have given numbers more or less discordant; so that, without giving any opinion on their different results, this question may be considered at present far from settled. M. Regnault himself does not give the law by which the ratio of heat varies with reference to the pressure, a subject involving questions of which experiments on the mechanical effects of elastic fluids seem to offer the most promising means of solution. In a former edition I inclined to this opinion from the greater visible effects of small changes in temperature on gases than on solids. When we read on a mercurial thermometer to the 1,000th or even 100th of a degree, and aim at quantitative results, errors may well arise from the minuteness of the measurement, but with gases minute differences of temperature are cognizable and measurable. Messrs. Joule and Thomson, however, in a paper on the thermal effects of fluids in motion, 1854, thus express themselves on the relation between the heat evolved and the work spent in compressing a gas kept at a constant temperature:—'This relation is not a relation of simple mechanical equivalence, as was supposed by Mayer in his "Bemerkungen ueber die Kräfte der Unbeleben Natur," in which he founded on it an attempt to evaluate numerically the mechanical equivalent of the thermal unit. The heat evolved may be less than, equal to, or greater than the equivalent of the work spent, according as the work pro-

duces other effects in the fluid than heat, produces only heat, or is assisted by molecular forces in generating heat, and according to the quantity of heat, greater than, equal to, or less than that held by the fluid in its primitive condition, which it must hold to keep itself at the same temperature when compressed. The *à priori* assumption of equivalence for the case of air, without some special reason from theory or experiment, is not less unwarrantable than for the case of any fluid whatever subjected to compression. Yet it may be demonstrated that water below its temperature of maximum density (39·1 Fahrenheit), instead of evolving any heat at all when compressed actually absorbs heat, and at higher temperatures evolves heat in greater or less, but probably always very small, proportion to the equivalent of the work spent; while air, as will be shown presently, evolves always, at least when kept at any temperature between 0° and 100° cent., somewhat more heat than the work spent in compressing it could alone create.'

These remarks show some of the difficulties, among many, which present themselves in the way of ascertaining a mechanical equivalent of heat; the most striking is perhaps the expansion of water, and some other substances, at a certain temperature by cold, or, if the expression be preferred, by the absorption of heat by a cold body.

Thus, assume a cylinder of water at its point of maximum density, 39°, which water supports a piston with a weight on it. Now let a substance colder than the water, say ice at 25°, be brought into contact with the cylinder, the water will expand by the cooling effect of the ice, and produce mecha nical force, raising the weight.

An engine the converse of the ordinary steam or calorific engines might thus be constructed, in which cold would be apparently the motive power, and instead of a furnace supplying the power, and the steam or air being abstracted by cold water, ice might act as the initiating power, and warm water be used as the abstractor. Even here the effects may be viewed as caused by an abstraction of heat; but they are not the less the converse of the ordinary thermal effects. Cold may be produced by mechanical force, *e.g.* the compres-

sion of water between 32° and 39°, or by the expansion of gases. It seems difficult to ascertain that in any experiments some cold is not produced by elongation, expansion, or other molecular change, which may neutralise some of the heat produced. To call the work done by ice in cooling water a mechanical effect of heat, the calorific sign must be changed; and though the phenomena may be entirely consistent with the conservation of force, and no force may be created or destroyed, the work done by the water, apparently expanded by cold, is in the same direction to that which is effected by heat, when the substance is above or below the given temperature. I have stated in previous editions of this work that 'Although, taking the phenomena as they are known to exist, the mechanical laws may be deduced, yet in any physical conception of the nature of heat expansion by cold has always been a great stumbling-block to me, and, I believe, to many others;' and, although it may be true, that, assuming a given amount of mechanical force to be all converted into heat, or a given amount of heat into mechanical force, there must be a relation of equivalence, I feel by no means certain that the true equivalent of heat for arrested motion has yet been attained. Mechanical force is now used in the arts for producing cold, and although it doubtless at the same time produces heat, the cold, if it be not equivalent to the heat, has to be deducted or estimated in calculating the result. It may turn out that in some cases it is a more correct expression to say that mechanical force produces a disturbance of the equilibrium of temperature than that it produces heat.

Molecular vibrations also, which cannot be called heat (just as undulations of a certain slowness or rapidity cannot be called sound or light), carry off some of the force employed, and electrical currents or inductive effects may convey away some, so that to say that a given dynamic force has, in any experiments hitherto made, been entirely converted into heat, and heat only, is a proposition of which, to my mind at least, there is not conclusive evidence.

To make it sure that a given motion of a given body has all been converted into heat, the motion itself should be

entirely arrested; any residual motion has to be deducted from the producing force. Experiments to verify this are difficult of execution, and even of conception. If a weight, the descent of which produces friction, fall very rapidly with slight resistance, the heat from friction is very slight, and the impact when the weight reaches the ground and force thence dispersed is great. If it fall with extreme slowness, the heat is also experimentally slight, for the cooling effects of surrounding matter go on nearly *pari passu* with the heating; there is, therefore, a practical degree of velocity at which the measured heat is a maximum; but how can it, in any given experimental investigation, be affirmed with certainty that this point has been attained, and if so, that the heat so measured is the real mechanical equivalent; motion not being actually arrested, and the dispersion of force, by the residual impact on the weight reaching its destination, being almost incapable of measurement in terms of heat?

I have endeavoured to give a proof (by showing the anomaly to which the contrary conclusion would lead) that, whatever amount of mechanical power is produced by one mode of application of heat, the same should, in theory, be equally produced by any other mode. But in practice the difference is immense; and therefore it becomes a question of great interest practically to ascertain what is the most convenient medium on which to apply the heat employed, and the best machinery for economising it. One great problem to be solved is the saving of the heat which the steam in ordinary engines, after having done its work, carries into the condenser, or, in the high-pressure engine, into the air. It is argued you have a large amount of fuel consumed to raise water to the boiling-point, at which its efficiency as a motive agent commences. After it has done a small portion of work, and while it still retains a very large portion of the heat originally communicated to it, you reject it, and have to start again with a fresh portion of steam which has similarly exhausted fuel—in other words, you throw away all, and more than all the heat which has been employed in raising the water to the boiling-point. Various plans have been devised to remedy this. Using again the warm water of the

condenser to feed the boiler, regains a part, but a very small part, of the heat. Employing the steam first for a high pressure, and then before its rejection or condensation using it for a low pressure cylinder, is a second mode; a third is to use the steam, after it has done its work on the piston, as a source of heat or second furnace, to boil ether, or some liquid which evaporates at a lower temperature than water. These plans have certain advantages; but the complexity of apparatus, the danger from combustion of ether, and other reasons, have hitherto precluded their general adoption. Under the term 'regenerating engine' various ingenious combinations have lately been suggested, and some experimental engines tried, with what success it is perhaps too early at present to pronounce an opinion. The fundamental notion on which this class of engine is based is that the vapour or air, when it has performed a certain amount of work, as by raising a piston, should, instead of being condensed or blown off, be retained and again heated to its original high temperature, and then used *de novo*; or that it should impart its heat to some other substance, and the latter in turn impart it to the fresh vapour about to act. The latter plan has been proposed by Mr. Ericsson: he passes the air which has done its work through layers of wire gauze, which are heated by the rejected air, and through which the next charge of air is made to pass. M. Seguin and Mr. Siemens have constructed machines upon the former principle, which are said to have given good experimental results. There is, however, a theoretical difficulty in all these, not affecting their capability of acting, but affecting the question of economy, which it does not seem easy to escape from. Whether the heated air or vapour be retained, or whether it yield its heat to a metallic or other substance, this heat must exercise its usual repulsive force, and this must re-act either against the returning piston or against the incoming vapour, and require a greater pressure in that to neutralise it. Vapour raising a piston and producing mechanical force effects this with decreasing power in proportion as the piston is moved. At a certain point the piston is arrested, or the stroke, as it is termed, is completed, but there is still compressed vapour in the cylinder capable

of doing work, but so little that it is, and must in practice, be neglected; if this compressed vapour be retained, the piston cannot be depressed without an extra force capable of overcoming the resistance of this, so to speak, semi-compressed vapour, in addition to that which is requisite to produce the normal work of the machine; and in whatever way the residual force be retained, it must either be antagonised at a loss of power for the initial force, or at most can only yield the more feeble power which it would have originally given if it had been allowed to act for a longer stroke on the piston. It may be that a portion of this residual force may be economised; indeed, this is done when the boiler is charged with warm water from the condenser, instead of with cold water; but some, indeed a notable loss, seems inevitable.

Without farther discussing the various inventions and theories on this subject, which are daily receiving increased development, it may be well to point out how far nature distances art in its present state. According to some careful estimates, the most economical of our furnaces consume from ten to twenty times as much fuel to produce the same quantity of heat as an animal produces; and Matteucci found that, from a given consumption of zinc in a voltaic battery, a far greater mechanical effect could be produced by making it act on the limbs of a recently-killed frog, notwithstanding the manifold defects of such an arrangement and its inferiority to the action of the living animal, than when the same battery was made to produce mechanical power, by acting on an electro-magnetic or other artificial motor apparatus. The ratio in his experiments was nearly six to one. Thus in all our artificial combinations we can but apply natural forces, and with far inferior mechanism to that which is perceptible in the economy of nature.

> Nature is made better by no mean;
> But nature makes that mean; so o'er that art,
> Which you say adds to nature, is an art
> That nature makes.

The term 'nature' being, in fact, an abstract expression for the changes and adaptations which have been brought

about in an immeasurably long time by the mutual interactions of physical forces, the which, by eliminating impediments, have produced a machinery more perfect than the rapid performances of art. Even with inorganic forces the perfection of natural action and natural selection is wonderful. The following instance may help to explain my meaning:—In some of the fissures of the limestone rocks to the west of Oystermouth, near Swansea, may be seen frills formed by bleached oyster shells, which in course of ages have been washed by the tide into them. So perfect is the filling up of these fissures that hardly a crevice is left between the shells, and so close the fitting that great force is required to remove a single shell. A frill of many yards' length is thus formed which it would much puzzle a human artificer to imitate.

A speculation has been thrown out by Sir W. Thomson, that, as a certain amount of heat results from mechanical action, chemical action, &c., and these other forces resolve themselves into heat, as this heat is radiated into space, there must be a gradual diminution of temperature for the earth, by which expenditure, however slow, being continuous, it would ultimately be cooled to a degree incompatible with the existence of animal and vegetable life—in short, that the earth and the planets of our system are parting with more heat than they receive, and are therefore progressively cooling. Geological researches seemed at first to support this view, as they showed that the climate of many portions of the terrestrial surface was at remote periods hotter than at the present time: the animals whose fossilised remains are found in ancient strata have their organism adapted to what we should now term a hot climate.* Mr. Balfour Stewart sums up the views of Thomson as follows:—'We are led to look at a beginning in which the particles of matter were in a diffuse chaotic state, but endowed with the power of gravitation, and we are led to look to an end in which the whole universe will be one equally heated inert mass, and from which everything like life or motion or beauty will have utterly gone away.' I have some difficulty in seeing how, on

* See, however, Mr. Croll's views, *post*.

this supposition, without creation of force, the universe got out of the diffuse chaotic state; for its tendency must, *ex hypothesi*, have always been to remain in or return to it; and if all other forces resolve themselves into heat, and heat is nowhere re-concentrated, but disperses itself through the universe until all acquires an uniform temperature, the theory of the Conservation of Energy is erroneous, for, as at the ultimate equal distribution all will be immobile and unchangeable, every year, month, hour or minute that state is being gradually approached, and so every minute some amount of active force is being lost; thus force, or energy (the term preferred) is not 'conserved,' but is in gradual progress of neutralisation, which virtually amounts to annihilation of force. There are so many circumstances of difficulty attending cosmical speculations, that but little reliance can be placed upon the most profound. We know not the original source of terrestrial heat, still less that of solar heat; we know not whether or not systems of planets may be so constituted as to communicate forces, *inter se*, so that forces which have hitherto escaped detection may be in a continuous or recurring state of interchange.

The movements produced by mutual gravitation may be the means of calling into existence molecular forces within the substances of the planets themselves. As neither from observation, nor from deduction, can we fix or conjecture any boundary to the universe of stellar orbs, as each advance in telescopic power gives us a new shell, so to speak, of stars, we may regard our globe, in the limit, as surrounded by a sphere of matter radiating and absorbing heat, light, and possibly other forces.

Such stellar radiations would not, from the evidence we have at present, appear sufficient to supply the loss of heat by terrestrial radiations; but it is quite conceivable that the whole solar system may pass through portions of space having different temperatures, as was suggested, I believe, by Poisson; that as we have a terrestrial summer and winter, so there may be a solar or systematic summer and winter, in which case the heat lost during the latter period might be restored during the former. The amount of the radiations

of the celestial bodies may again, from changes in their positions, vary through epochs which are of enormous duration as regards the existence of the human species; and while some are giving off heat, others, differently constituted, may be absorbing it. Mr. Matthieu Williams has suggested that the sun, in its motion through space (a motion of which there is now tolerably good evidence), condenses and brings within the range of chemical affinity and combustion the attenuated matter which fills the interplanetary spaces. As to whether this view will turn out to be well-founded or not it would be premature to express an opinion. That the change of position of the sun in space may produce some effect of condensation or friction on the interplanetary medium and on meteoric bodies interspersed through it, seems not improbable; and however visionary conjectures on the illimitable universe may be, the notion that the universe is gradually equalising its heat and tending to a chaos of uniform temperature and equilibrium of force, although it may be supported by the phenomena we see immediately around us, seems to me like those views of the instability of the solar system which Laplace negatived, and would require far more cogent proof than any at present given. Though the whole subject will probably never be grasped by human intelligence, enlarged observation may prove that phenomena seeming to tend in one direction will turn out to be recurrent, though never absolutely identical in their recurrence: that there is, throughout the universe, gradual change but no finality.

The views of Laplace, adopted by many, supposed the planets to have been formed by a gradual condensation of nebulous matter. A modification of this view might, perhaps, be suggested, viz. that worlds or systems, instead of being created as wholes at definite periods, are gradually changing by atmospheric additions or subtractions, or by accretions or diminutions arising from nebulous substance or from meteoric bodies, so that no star or planet could at any time be said to be created or destroyed, or to be in a state of absolute stability, but that some may be increasing, others dwindling away, and so throughout the universe, in the past as in the future. When, however, questions relating to cosmogony, or

to the beginning or end of worlds, are contemplated from a physical point of view, the period of time over which our experience, in its most enlarged sense, extends, is so indefinitely minute with reference to that which must be required for any notable change, even in our own planet, that a variety of theories may be framed equally incapable of proof or of disproof. We have no means of ascertaining whether many changes, which endure in the same direction for a term beyond the range of human experience, are really continuous or only secular variations, which may be compensated for at periods far beyond our ken, so that in such cases the question of comparative stability or change can at best be only answered as to a term which, though enormous with reference to our computations, sinks into nothing with reference to cosmical time, if cosmical time be not eternity. Subjects such as these, though of a kind on which the mind delights to speculate, appear, with reference to any hope of attaining reliable knowledge, far beyond the reach of any present or immediately prospective capacity of man.

ELECTRICITY.

ELECTRICITY is that affection of matter or mode of force which most distinctly and beautifully brings into relation other modes of force, and exhibits to a great extent, in a quantitative form, its own relation with them, and their reciprocal relations with it and with each other. From the manner in which the peculiar force called electricity is seemingly transmitted through certain bodies, such as metallic wires, the term *current* is commonly used to denote its apparent progress. It is very difficult to present to the mind any theory which will give a definite conception of its *modus agendi*: the early theories regard its phenomena as produced either by a single fluid idio-repulsive but attractive of all matter, or else as produced by two fluids, each idio-repulsive but attractive of the other. No substantive theory has been proposed other than these two; but although this is the case, I think I shall not be unsupported by many who have attentively studied electrical phenomena, in viewing them as resulting, not from the action of a fluid or fluids, but as a molecular polarisation of ordinary matter, or as matter acting by attraction and repulsion in a definite direction. Thus, the transmission of the voltaic current in liquids is viewed by Grotthus as a series of chemical affinities acting in a definite direction; for instance, in the electrolysis of water, *i.e.* its decomposition when placed between the poles or electrodes of a voltaic battery, a molecule of oxygen is supposed to be displaced by the exalted attraction of the neighbouring electrode; the hydrogen liberated by this displacement unites with the oxygen of the contiguous molecule of water; this in turn liberates its hydrogen, and so on; the current being nothing else than this molecular transmission of chemical affinity.

There is strong reason for believing that, with some exceptions, such as fused metals, liquids do not conduct electricity without undergoing decomposition; for even in those

extreme cases where a trifling effect of conduction is apparently produced without the usual elimination of substances at the electrodes, the latter, when detached from the circuit, show, by the counter-current which they are capable of producing when immersed in a fresh liquid, that their superficial state has been changed, doubtless by the determination to the surfaces of minute layers of substances having opposite chemical characters. The question whether or not a minute conduction in liquids can take place unaccompanied by chemical action, has however been much agitated, and may be regarded as *inter apices* of the science.

Assuming for the moment electrolysis to be the only known electrical phenomenon, electricity would appear to consist in transmitted chemical action. All the evidence we have is, that a certain affection of matter or chemical change takes place at certain distant points of space, the change at one point having a definite relation to the change at the other, and being capable of manifestation at any intermediate points.

If, now, the electrical effect called induction be examined, the phenomena will be found equally opposed to the theory of a fluid, and consistent with that of molecular polarisation. When an electrified conductor is brought near another which is not electrified, the latter becomes electrified by influence or induction, as it is termed, the nearest parts of each of these two bodies exhibiting states of electricity of the contrary denominations. Until this subject was investigated by Faraday, the intervening non-conducting body or dielectric was supposed to have no influence, and the effect was attributed to the repulsion at a distance of the electrical fluid. Faraday showed that these effects differed greatly according to the dielectric that was interposed. Thus they were more exalted with sulphur than with shellac ; more with shellac than with glass, &c. Matteucci, though differing from Faraday as to the explanation he gave, added some experiments which prove that the intervening dielectric is molecularly polarised. Thus a number of thin plates of mica are superposed like a pack of cards : metallic plates are applied to the outer facings, and one of them electrified, so that the apparatus is charged like a Leyden phial. Upon separating the plates with insulating

handles, each plate is separately electrified, one side of it being positive and the other negative, showing very neatly and decisively a polarisation throughout the intervening substances by the effect of induction.

Indeed, chemical action or electrolysis may, as I have shown, be transmitted by induction across a dielectric substance, such as glass, but apparently only while the glass is being charged with electricity. A wire passing through and hermetically sealed into a glass tube, a short portion only projecting, is made to dip into water contained in a Florence flask; the flask is immersed in water to an equal depth with that within it; the wire and another similar wire dipping into the outer water are made to communicate metallically with the powerful electrical machine known as Rhumkorf's coil; bubbles of gas instantly ascend from the exposed portions of the wires, but cease after a certain time, and are renewed when, after an interval of separation, the coil is again connected with the wires.

The following interesting experiment by Mr. Karsten affords evidence in corroboration of the molecular changes consequent upon electrisation: A coin is placed on a pack of thin plates of glass, and then electrified. On removing the coin and breathing on the glass plate, an impression of the coin is perceptible; this shows a certain molecular change on the surface of the glass opposed to the plate, or of the vapours condensed on such surface. This effect might and has been interpreted as arising from a film of greasy deposit supposed to exist on the plate; the impressions, however, have been proved to penetrate to certain depths below the surface, and not to be removed by polishing.

The following result, however, goes farther: On separating carefully the glass plates, images of the coin can be developed on each of the surfaces, showing that the molecular change has been transmitted through the substance of the glass; and we may thence reasonably suppose that a piece of glass, or other dielectric body, if it could be split up while under the influence of electric induction, would exhibit some molecular change at each side of each lamina, however minute the subdivision.

I have succeeded in farther extending this experiment, and in permanently fixing the images thus produced by electricity. Between two carefully cleaned glass plates is placed a word or device cut out of paper or tinfoil; sheets of tinfoil a little smaller than the glass plates are placed on the outside of each plate, and these coatings are brought into contact with the terminals of a Rhumkorf coil. After electrisation for a few seconds, the glasses are separated, and their interior surfaces exposed to the vapour of hydrofluoric acid, which acts chemically on glass; the portions of the glass not protected by the paper device are corroded, while those so protected are untouched or less affected by the acid, so that a permanent etching is thus produced, which nothing but disintegration of the glass will efface.

Some farther experiments of mine on this subject bring out in a still more striking manner these curious molecular changes. One of the plates of glass having been electrified in the manner just mentioned, is coated, on the side impressed with the invisible electrical image, with a film of iodised collodion in the manner usually adopted for photographic purposes; it is then in a dark room immersed in a solution of nitrate of silver; then exposed to diffuse light for a few seconds. On pouring over the collodion the usual solution of pyrogallic acid, the invisible electrical image is brought out as a dark device on a light ground, and can be permanently fixed by hyposulphite of soda. The point worthy of observation in this experiment is, that this permanent image exists in the collodion film, which can be stripped off the glass, dried, and placed on any other surface, so that the molecular change consequent on electrisation has communicated, by contact or close proximity, a change to the film of collodion corresponding in form with that on the glass, but undoubtedly of a chemical nature. Electricity has, moreover, in this experiment so modified the surface of glass, that it can, in its turn, modify the structure of another substance so as to alter the relation of the latter to light. It would require a curious complication of hypothetic fluids to explain this; but if electricity and light be supposed to be affections of ordinary ponderable matter, the difficulty is only one of detail.

If, again, we examine the electricity of the atmosphere, when, as is usually the case, it is positive with respect to that of the earth, we find that each successive stratum is positive to those below it and negative to those above it; and the converse is the case when the electricity of the atmosphere is negative with respect to that of the earth.

If other electrical phenomena be selected, other changes in the matter affected will be found to have taken place. The electric spark, the brush, and similar phenomena, the old theories regarded as actual emanations of the matter or fluid, Electricity; I venture to regard them as produced by an emission of the material itself from whence they issue, and a molecular action of the gas, or intermedium, through or across which they are transmitted.

The colour of the electric spark, or of the voltaic arc (*i.e.* the flame which plays between the terminal points of a powerful voltaic battery), is dependent upon the substance of the metal, subject to certain modifications of the intermedium: thus, the electric spark or arc from zinc is blue; from silver, green; from iron, red and scintillating; precisely the colours afforded by these metals in their ordinary combustion. A portion of the metal is also found to be actually transmitted with every electric or voltaic discharge: in the latter case, indeed, where the quantity of matter acted upon is greater than in the former, the metallic particles emitted by the electrodes or terminals can be readily collected, tested, or even weighed. Moreover, the transverse lines in the spectrum of the electric discharge differ for different metals used as terminals. It would thus appear that the electrical discharge arises, at least in part, from an actual repulsion and severance of the electrified matter itself, which flies off at the points of least resistance.

A careful examination of the phenomena attending the electric spark or the voltaic arc, which latter is the electric disruptive discharge acting on greater portions of matter, tends to modify considerably our previous idea of the nature of the electric force as a producer of ignition and combustion. The voltaic arc is perhaps, strictly speaking, neither ignition nor combustion. It is not simply ignition; because the

matter of the terminals is not merely brought to a state of incandescence, but is physically separated and partially transferred from one electrode to another, much of it being dissipated in a vaporous state. It is not combustion; for the phenomena will take place independently of atmospheric air, oxygen gas, or any of the bodies usually called supporters of combustion, combustion being in fact chemical union attended with heat and light. In the voltaic arc we may have no chemical union; for if the experiment be performed in an exhausted receiver, or in nitrogen, the substance forming the electrodes is condensed, and precipitated upon the interior of the vessel in, chemically speaking, an unaltered state. Thus, to take a very striking example: if the voltaic discharge be taken between zinc terminals in an exhausted receiver, a fine black powder of zinc is deposited on the sides of the receiver; this can be collected, and takes fire readily in the air by being touched with a match, or ignited wire, instantly burning into white oxide of zinc. To an ordinary observer, the zinc would appear to be burned twice—first in the receiver, where the phenomenon presents all the appearance of combustion, and secondly in the real combustion in air. With iron the experiment is equally instructive. Iron is volatilised by the voltaic arc in nitrogen or in an exhausted receiver; and when a scarcely perceptible film has lined the receiver, this is washed with an acid, which then gives, with ferrocyanide of potassium, the Prussian-blue precipitate. In this case we readily distil iron, a metal by ordinary means *fusible* only at a very high temperature.

Another strong evidence that the voltaic discharge consists of the material itself of which the terminals are composed, is the peculiar rotation which is observed in the light when iron is employed, the magnetic character of this metal causing its molecules to rotate by the influence of the voltaic current.

If we increase the number of reduplications in a voltaic series, we increase the length of the arc, and also increase its intensity or power of overcoming resistance. With a battery consisting of a limited number, say 100 reduplications, the discharge will not pass from one terminal to the other without first bringing them into contact, but if we increase the number

of cells to 400 or 500, the discharge will pass from one terminal to the other before they are brought into contact. The difference between what is called franklinic electricity, or that produced by an ordinary electrical machine, and voltaic electricity, or that produced by the ordinary voltaic battery, is that the former is of much greater intensity than the latter, or has a greater power of overcoming resistance; but, assuming an equal initial power, it acts upon a much smaller quantity of matter. If, then, a voltaic battery be formed with a view to increase the intensity and lessen the quantity, the character of the electrical phenomena approximates those of the electrical machine. In order to effect this, the sizes of the plates of the battery, and thence the quantity of matter acted on in each cell, must be reduced, but the number of reduplications increased. Thus if in a battery of 100 pairs of plates each plate be divided, and the battery be arranged so as to form 200 pairs, each being half the original size, the quantitative effects are diminished, and the effects of intensity increased. By carrying on this sub-division, diminishing the sizes and increasing the number, as is the case in the voltaic piles of Deluc and Zamboni, effects are ultimately produced similar to those of franklinic electricity, and we thus gradually pass from the voltaic arc to the spark or electric discharge.

This discharge, as I have already stated, has a colour depending in part upon the nature of the terminals employed. If these terminals be highly polished, a spot will be observed, even in the case of a small electric spark, at the points from which the discharge emanates. The matter of the terminals is itself affected; and a transmission of this matter across the intervening space is detected by the deposition of minute quantities of the metal or substance composing the one upon the other terminal.

If the gas or elastic medium between the terminals be changed, a change takes place in the length or colour of the discharge, showing an affection of the intervening matter. If the gas be rarefied, the discharge gradually changes with the degree of rarefaction, from a spark to a luminous glow or diffuse light, differing in colour in different gases, and capable

of extending to a much greater distance than when it takes place in air of the ordinary density. Thus, in highly-attenuated air a discharge may be made to pass across six or seven feet of space, while in air of the ordinary density it would not pass across an inch. An observer regarding the beautiful phenomena exhibited by this electric discharge in attenuated gas, which, from some degree of similarity in appearance to the Aurora Borealis, has been called the electric Aurora, would have some difficulty in believing such effects could be due to an action of ordinary matter. The amount of gas present is extremely small ; and the terminals, to a cursory examination, show no change after long experimenting. It is therefore not to be wondered at that the first observers of this and similar phenomena regarded electricity as in itself something—as a specific entity or fluid. Even in this extreme case, however, upon a more careful examination we shall find that a change does take place, both as regards the gas and as regards the terminals. Let one of these consist of a highly-polished metal—a silver plate is one of the best materials for the purpose—and let the discharges in attenuated atmospheric air take place from a point, say a common sewing-needle, to the surface of the polished silver plate ; it will be found that this is gradually changed in appearance opposite the point—it is oxidated, and gradually more and more corroded as the discharge is continued.

If now the gas be changed, and highly-rarefied hydrogen be substituted for the rarefied air, all other things remaining the same, upon passing the discharges as before the oxide will be cleared off the plate, and the polish to a great extent restored—not entirely, because the silver has been disintegrated by the oxidation—and the portion which has been affected by the discharge will present a somewhat different appearance from the remainder of the plate.

A question will probably here occur to the reader :—What will be the effect if there be not an oxidating medium present, and the experiment be first performed in a rarefied gas which possesses no power of chemically acting on the plate? In this case there will still be a molecular change or disintegration of the plate ; the portion of it acted on by the discharge

will present a different appearance from that which is beyond its reach, and a whitish film, somewhat similar to that seen on the mercurialised portions of a daguerreotype, will gradually appear on the portion of the plate affected by the discharge. If the gas be a compound, as carbonic oxide, or a mixture, as oxygen and hydrogen, and consequently contain elements capable of producing oxidation and reduction, then the effect upon the plate will depend upon whether it be positive or negative; in the former case it will be oxidated, in the latter the oxide, if existing, will be reduced. This effect will also take place in atmospheric air, if it be highly rarefied, and can hardly be explained otherwise than by a molecular polarisation of the compound gas. If, again, the metal be reduced to a small point, and be of such material that the gas cannot act chemically upon it, it can yet be shown to be disintegrated by the electric spark. Thus, let a fine platinum wire be hermetically sealed in a glass tube, and the extremity of the tube and the wire ground to a flat surface, so as to expose a section only of the wire; after taking the discharge from this for some time, it will be found that the platinum wire is worn away, and that its termination is sensibly below the level of the glass. If the discharges from such a platinum wire be taken in gas contained in a narrow tube, a cloud or film consisting of a deposit of platinum will be seen on the part of the tube surrounding the point.

Another curious effect which, in addition to the above, I have detected in the electrical discharge in attenuated media, is that when passing between terminals of a certain form, as from a wire placed at right angles to a polished plate, the discharge possesses certain phases or fits of an alternate character, so that, instead of impressing an uniform mark on a polished plate, a series of concentric rings is formed.

Priestley observed that, after the discharge of a Leyden battery, rings consisting of fused globules of metal were formed on the terminal plates; in my experiments made in attenuated media, alternate rings of oxidation and deoxidation are formed. Thus, if the plate be polished, coloured rings of oxide will alternate with rings of polished or unoxidated surface; and if the plate be previously coated with an uniform

film of oxide, the oxide will be removed in concentric spaces, and increased in the alternate ones, showing a lateral alternation of positive and negative electricity, or electricity of opposite character in the same discharge.

It would be hasty to assert that in no case can the electrical disruptive discharge take place without the terminals being affected. I have, however, seen no instance of such a result where the discharge has been sufficiently prolonged, and the terminals in such a state as could be expected to render manifest slight changes.

The next question which would occur in following out the enquiry which has been indicated would probably be :—What is the action upon the gas itself? Is this changed in any manner?

In answer to this, it must be admitted that, in the present state of experimental knowledge on this subject, certain gases only, appear to leave permanent traces of their having been changed by the discharge, while others, if affected by it—which, as will be presently seen, there are reasons to believe they are—return to their normal state immediately after the discharge.

In the former class we may place many compound gases, as ammonia, olefiant gas, protoxide of nitrogen, deutoxide of nitrogen, and others, which are decomposed by the action of the discharge. Mixed gases are also chemically combined by it: for instance, oxygen and hydrogen unite and form water; common air gives nitric acid; chlorine and aqueous vapour give oxygen, the chlorine uniting with the hydrogen of the water.

But, farther than this, in the case of certain elementary gases a permanent change is effected by the electrical discharge. Thus, oxygen submitted to the discharge is partially changed into the substance called ozone, a substance now considered to be an allotropic condition of oxygen; and there is reason to believe that, when the change takes place, there is a definite polar condition of the gas, and that definite portions of it are affected—that in a certain sense one portion of the oxygen bears temporarily to the other the relation which hydrogen ordinarily does to oxygen.

If the discharge be passed through the vapour of phos-

phorus in the vacuum of a good air-pump, a deposit of allotropic phosphorus soon coats the interior of the receiver, showing an analogous change to that produced in oxygen; and in this case a series of transverse bands or stratifications appear in the discharge, showing a most striking alteration in its physical character, dependent on the medium across which it is transmitted. These effects were first observed by me in the year 1852. They have since been much examined by Continental philosophers, and much extended by Mr. Gassiot; but no entirely satisfactory *rationale* of them has yet been given.

There are many gases which either do not show any permanent change, or (which is more probably the case) the changes produced in them by the electrical discharge have not yet been detected. Even with these gases, however, the difference of colour, of length, or of the different position of a certain dark space or spaces which appear in the discharge, shows that the discharge differs for different media. We never find that the discharge has itself added to or subtracted from the total weight of the substances acted on: we find no evidence of a fluid but the visible phenomena themselves; and those we may account for by the change which takes place in the matter affected.

I have here, as elsewhere, used words of common acceptation, such as 'matter affected by the discharge,' &c., though, upon the view I am suggesting, the discharge is itself this affection of matter; and the writing these passages affords, to me at least, a striking instance of how much ideas are bound up in words, when, to express a view differing from the received one, words involving the received one are necessarily used.

I pass now to the effect of the transmission of electricity by the class of the best conducting bodies, such as the metals and carbon; here, though we cannot at present give the exact character of the motion impressed upon the particles, there are yet many experiments which show that a change takes place in such substances when they are affected by electricity.

Let discharges from a Leyden jar or battery be passed through a platinum wire, too thick to be fused by the dis-

charges, and free from constraint, it will be found that the wire is shortened; it has undergone a molecular change, and apparently been acted on by a force transverse to its length. If the discharges be continued, it gradually gathers up in small irregular bends or convolutions. So with voltaic electricity: place a platinum wire in a trough of porcelain, so that when fused it shall retain its position as a wire, and then ignite it by a voltaic battery. As it reaches the point of fusion it will snap asunder, showing a contraction in length, and consequently a distension or increase in its transverse dimensions. Perform the same experiment with a lead wire, which can be more readily kept in a state of fusion, and follow it, as it contracts, by the terminal wires of the battery; it will be seen to gather up in nodules, which press on each other like a string of beads of a soft material which have been longitudinally compressed.

As we increase the thickness of the wires in these experiments with reference to the electrical force employed, we lessen the perceptible effect; but even in this case we shall be enabled safely to infer that some molecular change accompanies the transmission of electricity: the wires are heated in a degree decreasing as their thickness increases—but by increasing the delicacy of our tests as the heating effects decrease in intensity, we may indefinitely detect the augmentation of temperature accompanying the passage of electricity—and wherever there is augmentation of temperature there must be expansion or change of position of the molecules.

Again, it has been observed that wires which have for a long time transmitted electricity, such as those which have served as conductors for atmospheric electricity, have their texture changed, and are rendered brittle. In this observation, however, though made by a skilful electrician, M. Peltier, the effects of exposure to the atmosphere, to changes of temperature, &c., have not been sufficiently eliminated to render it worthy of entire confidence. There are, however, other experiments which show that the elasticity of metals is changed by the passage through them of the electric current.

Thus, M. Wertheim has, from an elaborate series of experiments, arrived at the conclusion that there is a tem-

porary diminution in the coefficient of elasticity in wires while they are transmitting the electric current, which is independent of the heating effect of the current.

M. Dufour has made a considerable number of experiments with the view of ascertaining if any permanent change in metals is effected by electrisation. He arrives at the curious result that in a copper wire through which a feeble voltaic current has passed for several days, a notable diminution in tenacity takes place ; while, in an iron wire, the tenacity is increased ; and that these effects were more perceptible when the wires had been electrised for a long time (nineteen days) than for a short time (four days). The copper wire was, in his experiment, not perfectly pure ; so that the effect or a portion of it, might be due to the state of alloy : in the case of iron, the magnetic character of the metal would probably modify the effects, and might account for the opposite character of the results with these two metals.

Matteucci has made experiments on the conduction of electricity by bismuth in directions respectively parallel or transverse to the planes of principal cleavage, and he finds that bismuth conducts electricity and heat better in the direction of the cleavage planes than in that transverse to them.

Many other experiments have been made both on the production of thermo-electric currents by two portions of the same crystalline metal, but with the planes of crystallisation arranged in different directions relatively to each other, and also on the differences in conduction of heat and electricity according to the direction in which they are transmitted with reference to the planes of crystallisation ; the results varying with the arrangements.

It is found, moreover, that the slightest difference in homogeneity in the same metal enables it when heated to produce a thermo-electric current, and that metals in a state of fusion, in which state they may be presumed to be homogeneous throughout, give no thermo-electric current : thus, hot in contact with cold mercury has been shown by Matteucci to give no thermo-electric current, and the same is the case with portions of fused bismuth unequally heated.

The fact that the molecular structure or arrangement of a

body influences—indeed, I may say determines—its conducting power, is by no means explained by the theory of a fluid; but if electricity be only a transmission of force or motion, the influence of the molecular state is just what would be expected. Carbon, in a transparent crystalline state, as diamond, is as perfect a non-conductor as we know; while in an opaque amorphous state, as graphite or charcoal, it is one of the best conductors: thus, in the one state, it transmits light and stops electricity, in the other it transmits electricity and stops light.

It is a circumstance worthy of remark, that the arrangement of molecules, which renders a solid body capable of transmitting light, is most unfavourable to its transmission of electricity, transparent solids being very imperfect conductors of electricity; so gases readily transmit light, but are amongst the worst conductors of electricity, if indeed, properly speaking, they can be said to conduct at all.

The conduction of electricity by different classes of bodies has been generally regarded as a question of degree: thus metals were viewed as perfect conductors, charcoal less so, water and other liquids as imperfect conductors, &c. But, in fact, though between one metal and another the mode of transmission may be the same and the difference one of degree, a different molecular effect obtains, when we contrast metals with electrolytic liquids and these with gases.

Attenuated gases may be, in one sense, regarded as non-conductors, in another as conductors; thus, if gold-leaves be made to diverge, by electrical repulsion, in air at ordinary pressure, they in a short time collapse; while in highly-rarefied air, or what is commonly termed a vacuum, they remain divergent for days; and yet electricity of a certain degree of tension passes readily across attenuated air, and with difficulty across air of ordinary density.

Again, where the electrical terminals are brought to a state of visible ignition, there are symptoms of the transmission of electricity of low tension across gases; but no such effects have been detected at lower temperatures. All this presents a strong argument in favour of the transmission of electricity across gases being effected by convection in the disruptive

discharge, and not by a conduction similar to that which takes place either with metals or with electrolytes.

The ordinary attractions and repulsions of electrified bodies present no more difficulty when regarded as being produced by a change in the state or relations of the matter affected, than do the attractions of the earth by the sun, or of a leaden ball by the earth; the hypothesis of a fluid is not considered necessary for the latter, and need not be so for the former class of phenomena. How the phenomena are produced to which the term attraction is applied is still a mystery. Newton, speaking of it, says: 'What I call attraction may be performed by impulse, or by some other means unknown to me. I use that word here to signify only in general any force by which bodies tend towards one another, whatsoever be the cause.' If we suppose a fluid to act in attractions and repulsions, the imponderable fluid must drag or push the matter with it: thus when we feel a stream of air rushing from an electrified metallic point, each molecule of air contiguous to the point being repelled, another takes its place, which is in its turn repelled;—how does a hypothetic fluid assist us here? If we say the electrical fluid repels itself, or the same electricity repels itself, we must go farther and assert, that it not only repels itself, but either communicates its repulsive force to the particles of the air, or carries with it the particle of air in its passage. Is it not more easy to assume that the particle of air is in such a state that the ordinary forces which keep it in equilibrium are disturbed by the electrical force, or force in a definite direction communicated to it, and that thus each particle in turn recedes from the point? As this latter force is increased, not only does the particle of air which was contiguous to the metallic point recede, but the cohesion of the extreme particles of metal may be overcome to such an extent that these are detached, and the brush or spark may consist wholly or in part of minute particles of the metal itself thrown off. Of this there is some evidence, though the point can hardly be considered as proved. A similar effect undoubtedly takes place with voltaic electricity, acting upon a terminal immersed in a liquid; thus if metallic terminals of a powerful voltaic battery be immersed

in water, metal, or the oxide of metal, is forcibly detached, producing great heat at the point of disruption.

If we apply ourselves to the effect of electricity in the animal economy, we find that the first *rationale* given of the convulsive effect produced by transmission through the living or recently killed animal was, that electricity itself, something substantive, passed rapidly through the body, and gave rise to the contractions; step by step we are now arriving at the conviction that consecutive particles of the nerves and muscles are affected. Thus the contractions which the prepared leg of a frog undergoes at the moment it is submitted to the voltaic current, cease after a time if the current be continued, and are renewed on breaking the circuit, *i.e.* at the moment when the current ceases to traverse it. The excitability of a nerve, moreover, or its power of producing muscular contraction, is weakened or destroyed by the transmission of electricity in one direction, while the excitability is increased by the transmission of electricity in the opposite direction; showing that the fibre or matter itself of the nerve is changed by electrisation, and changed in a manner bearing a direct relation to the other effects produced by electricity.

Portions of muscle and of nerve present different electrical states with reference to other portions of the same muscle or nerve; thus the external part of a muscle bears the same relation to the internal part as platinum does to zinc in the voltaic battery; and delicate galvanoscopes will show electrical effects when interposed in a conducting circuit connecting the surface of a nerve with its interior portions. Matteucci has proved that a species of voltaic pile may be formed by a series of slices of muscle, so arranged that the external part of one slice may touch the internal part of the next, and so on.

Lastly, the magnetic effects produced by electricity also show a change in the molecular state of the magnetic substance affected; as we shall see when the subject of magnetism is discussed.

I have taken in succession all the known classes of electrical phenomena; and, as far as I am aware, there is not a single electrical effect, where, if a close investigation be instituted, and the materials chosen in a state for exhibiting

minute changes, evidence of molecular change will not be detected; so that, excepting those cases where infinitesimally small quantities of matter are acted on, and our means of detection fail, electrical effects are known to us only as changes of ordinary matter. It seems to me as easy to imagine these changes to be effected by a force acting in definite directions, as by a fluid which has no independent or sensible existence, and which, it must be assumed, is associated with, or exerts a force acting upon ordinary matter, or matter of a different order from the supposed fluid. As the idea of the hypothetic fluid is pursued, it gradually vanishes, and resolves itself into the idea of force. The hypothesis of matter without weight presents in itself, as I believe, fatal objections to the theories of electrical *fluids*, which are entirely removed by viewing electricity as force or motion, and not as matter.

If it be said that the effects we have been considering may still be produced by a fluid, and that this fluid acts upon ordinary matter in certain cases, polarising the matter affected or arranging its particles in a definite direction, whilst in others, by its attractive or repulsive force, it carries with it portions of matter; then, if the fluid in itself be incapable of recognition by any test, if it be only evidenced by the changes which it operates in ponderable matter, the words fluid and force become identical in meaning; we may as well say that the attraction of gravitation or weight is occasioned by a fluid, as that electrical changes are so.

When, as is constantly done in common parlance, a house is said to be *struck*, windows *broken*, metals *fused* or *dissipated* by the electrical *fluid*, are not the expressions used such as, if not sanctioned by habit, would seem absurd? In all the cases of injury done by lightning there is no fluid perceptible; the so-called sulphurous odour is either ozone developed by the action of electricity on atmospheric air, or the vapour of some substance dissipated by the discharge; does it not then seem more consonant with experience to regard these effects as produced by force, as we have analogous effects produced by admitted forces, in cases where no one would invoke the aid of a hypothetic fluid for explanation? For instance, glasses may be broken by electrical discharges; so may they by sono-

rous vibrations. Metals electrified or magnetised will emit a sound; so they will if struck, or if a musical note with which they can vibrate in unison be sounded near to them.

Even chemical decomposition, in cases of feeble affinity, may be produced by purely mechanical effects. A number of instances of this have been collected by M. Becquerel; and substances whose constituents are held together by feeble affinities such as iodide of nitrogen and similar compounds, are decomposed by the vibration occasioned by sound.

If, instead of being regarded as a fluid or imponderable matter *sui generis*, electricity be regarded as the motion of an ether, equal difficulties are encountered. Assuming ether to pervade the pores of all bodies, is the ether a conductor or non-conductor? If the latter—that is, if the ether be incapable of transmitting the electrical wave—the ethereal hypothesis of electricity necessarily fails; but if the motion of the ether constitute what we call conduction of electricity, then the more porous bodies, or those most permeable by the ether, should be the best conductors. But this is not the case. If, again, the metal and the air surrounding it are both pervaded by ether, why should the electrical wave affect the ether in the metal, and not stir that in the gas? To support an ethereal hypothesis of electricity, many additional and hardly reconcilable hypotheses must be imported.

The fracture and comminution of a non-conducting body, the fusion or dispersion of a metallic wire by the electrical discharge, are effects equally difficult to conceive upon the hypothesis of an ethereal vibration, as upon that of a fluid, but are necessary results of the sudden subversion of molecular polarisation, or of a sudden or irregular vibratory movement of the matter itself. We see similar effects to those of electricity produced by sonorous vibrations, which might be called conduction and nonconduction of sound. One body transmits sound easily, another stops or deadens it, as it is termed—*i.e.* disperses the vibrations, instead of continuing them in the same direction as the primary impulse; and solid bodies may, as has been above observed, be shivered by sudden impulses of sound in those cases where all the parts of the body cannot uniformly carry on the undulatory motion.

The progressive stages in the History of Physical Philosophy will account in a great measure for the adoption by the early electricians of the theories of fluids.

The ancients, when they witnessed a natural phenomenon, removed from ordinary analogies, and unexplained by any mechanical action known to them, referred it to a soul, a spiritual or preternatural power; thus amber and the magnet were supposed by Thales to have a soul; the functions of digestion, assimilation, &c., were supposed by Paracelsus to be effected by a spirit (the Archæus). Air and gases were also at first deemed spiritual, but subsequently they became invested with a more material character; and the same words, *πνεῦμα*, spirit, &c., were used to signify the soul or a gas; the very word gas, from *geist*, a ghost or spirit, affords us an instance of the gradual transmission of a spiritual into a physical conception; the words 'give up the ghost,' 'expire,' had the same confusion of ideas, and the rough conception of death seemed to have been that the last long breath of the dying was the expulsion or separation of the soul from the body; indeed it is so represented in ancient illuminated missals, where a faint spectre is depicted as issuing from the mouth of the dying man.

The establishment by Torricelli of the ponderable character of air and gas, showed that substances which had been deemed spiritual and essentially different from ponderable matter were possessed of its attributes. A less superstitious mode of reasoning ensued, and now aëriform fluids were shown to be analogous in many of their actions to liquids or known fluids. A belief in the existence of other fluids, differing from air as this differed from water, grew up, and when a new phenomenon presented itself, recourse was had to a hypothetic fluid for explaining the phenomenon and connecting it with others; the mind once possessed of the idea of a fluid, soon invested it with the necessary powers and properties, and grafted upon it a luxurious vegetation of imaginary offshoots.

In what I am here throwing out I wish to guard myself from being supposed to state that the course of theory, historically viewed, followed exactly the dates of the discoveries

which were effectual in changing its character; sometimes a discovery precedes, at other times it succeeds a change in the general course of thought; sometimes, and perhaps most frequently, it does both—*i.e.* the discovery is the result of a tendency of the age and of the continually improved methods of observation, and when made, it strengthens and extends the views which have led to it. I think the phases of thought which physical philosophers have gone through will be found generally such as I have indicated, and that the gradual accumulation of discoveries which has taken place during the more recent periods, by showing what effects can be produced by dynamical causes alone, is rapidly tending to a general dynamical theory into which that of the imponderable fluids promises ultimately to merge.

Commencing with electricity as an initiating force, we get *motion* directly produced by it in various forms; for instance, in the attraction and repulsion of bodies, evidenced by mobile electrometers, such as that of Cuthbertson, where large masses are acted on; the rotation of the fly-wheel, another form of electrical repulsion, is also a mode of palpable, visible motion.

It would follow, from the reasoning in this essay, that when electricity performs any mechanical work which does not return to the machine, electrical power is lost. It would be unsuitable to the scope of this work to give the mathematical labours of M. Clausius and others here; but the following experiment, which I devised for making the result evident to an audience at the Royal Institution, will form a useful illustration:—A Leyden jar, of one square foot coated surface, has its interior connected with a Cuthbertson's electrometer, between which and the outer coating of the jar are a pair of discharging balls fixed at a certain distance (about half an inch apart). Between the Leyden jar and the prime conductor is inserted a small unit jar of nine square inches surface, the knobs of which are 0·2 inch apart.

The balance of the electrometer is now fixed by a stiff wire inserted between its knobs, and the Leyden jar charged by discharges from the unit jar. After a certain number of these, say twenty, the discharge of the large jar takes place across the half-inch interval. This may be viewed

as the expression of electrical power received from the unit jar. The experiment is now repeated, the wire between the knobs having been removed, and therefore the 'tip,' or the raising of the weight, is performed by the electrical repulsion and attraction of the two pairs of balls. At twenty discharges of the unit jar the balance is subverted, and one attracting knob drops upon the other; but *no discharge takes place*, showing that some electricity has been lost or converted into the mechanical power which raised the balance.

By another mode of expression the electricity may be supposed to be masked or analogous to latent heat, and it would be restored if the ball were brought back without discharge by extraneous force. If the discharge or other electrical effects were the same in both cases, then, since the raising of the ball or weight is an extra mechanical effort, and since the weight is capable by its fall of producing electricity, heat, or other force, it would seem that force could be got out of nothing, or perpetual motion obtained.

The above experiment is suggestive of others of a similar character, which may be indefinitely varied. Thus I have found that two balls made to diverge by electricity do not give to an electrometer the same amount of electricity as they do if, whilst similarly electrified, they are kept forcibly together. This experiment is the converse of the former one. There is an advantage in electrical experiments of this class as compared with those on heat, viz. that though there is no perfect insulation for electricity, yet our means of insulating it are immeasurably superior to any attainable for heat.

Electricity directly produces *heat*, as shown in the ignited wire, the electric spark, and the voltaic arc; in the latter, the most intense heat with which we are acquainted—so intense, indeed, that it cannot be measured—as every sort of matter is dissipated by it.

In the phenomenon of electrical ignition, as shown by a heated conjunctive wire, the relation of force and resistance, and the correlative character of the two forces, electricity and heat, are strikingly demonstrated. Let a thin wire of platinum join the terminals of a voltaic battery of suitable power, the wire will be ignited, and a certain amount of chemical action will

take place in the cells of the battery—a definite quantity of zinc being dissolved and of hydrogen eliminated in a given time. If now the platinum wire be immersed in water, the heat will, from the circulating currents of the liquid, be more rapidly dissipated, resistance in the wire is lessened, and we shall instantly find that the chemical action in the battery will be increased, more zinc will be dissolved, and more hydrogen eliminated for the same time; the heat being conveyed away by the water, more chemical action is required to generate it, just as more fuel is required for boiling in proportion as evaporation is more rapid.

Reverse the experiment, and instead of placing the wire in water, place it in the flame of a spirit-lamp, so that the force of heat meets with greater resistance to its dissipation. We now find that the chemical action is less than in the first or normal experiment. If the wire be placed in other different gaseous or liquid media, we shall find that the chemical action of the battery will be proportioned to the facility with which the heat is circulated or radiated by these media, and we thus establish an alternating reciprocity of action between these two forces: a similar reciprocity may be established between electricity and motion, magnetism and motion, and so of other forces. If it cannot be realised with all, it is probably because we have not yet eliminated interfering actions. If we carefully think over the matter, we shall, unless I am much mistaken, arrive at the conclusion that it cannot be otherwise, unless it be supposed that a force can arise from nothing—can exist without antecedent force.

In the phenomenon of the voltaic arc, the electric spark, &c., to which I have already adverted, electricity directly produces *light* of the greatest intensity of any artificially obtained. It directly produces *magnetism*, as shown by Oersted, who first distinctly proved the connection between electricity and magnetism. These two forces act upon each other, not in straight lines, as all other known forces do, but in a rectangular direction; that is, bodies affected by dynamic electricity, or the conduits of an electric current, tend to place magnets at right angles to them; and, conversely, magnets tend to place bodies conducting electricity at right angles to them. Thus an

electric current appears to have a magnetic action, in a direction cutting its own at right angles; or, supposing its section to be a circle, tangential to it; if, then, we reverse the position, and make the electric current form a series of tangents to an imaginary cylinder, this cylinder should be a magnet. This is effected in practice by coiling a wire as a helix or spiral, and this, when conducting an electrical current, is to all intents and purposes a magnet. A soft iron core placed within such a helix has the property of concentrating its power, and then we can, by connection or disconnection with the source of electricity, instantly make or unmake a most powerful magnet.

We may figure to the mind electrified and magnetised matter, as lines of which the extremities repel each other in a definite direction; thus, if a line A B represent a wire affected by electricity, and superposed on C D, a wire affected by magnetism, the extreme points A and B will be repelled to the farthest distances from the points C and D, and the line A B be at right angles to the line C D; and so, if the lines be subdivided to any extent, each will have two extremities or poles repulsive of those of the other. If the line of matter affected by electricity be a liquid, and consequently have entire mobility of particles, a continuous movement will be produced by magnetisation, each particle successively tending, as it were, to fly off at a tangent from the magnet: thus, place a flat dish containing acidulated water on the poles of a powerful magnet, immerse the terminals of a voltaic battery in the liquid just above the magnetic poles, so that the lines of electricity and of magnetism coincide; the water will now assume a movement at right angles to this line, flowing continuously, as if blown by an equatorial wind, which may be made east or west with reference to the magnetic poles by altering the direction of the electrical current: a similar effect may be produced with mercury. These cases afford an additional argument to those previously mentioned, of the particles of matter being affected by the forces of electricity and magnetism in a way irreconcilable with the fluid or ethereal hypothesis.

The representation of transverse direction by magnetism

and electricity appears to have led Coleridge to parallel it by the transverse expansion of matter, or length and breadth, though he injured the parallel by adding galvanism as depth; whether a third force exists which may bear this relation to electricity and magnetism is a question upon which we have no evidence.

The ratio which the attractive magnetic force produced bears to the electric current producing it has been investigated by many experimentalists and mathematicians. The data are so numerous and so variable, that it is difficult to arrive at definite results. Thus, the relative size of the coil and the iron, the temper or degree of hardness of the latter, its shape, or the proportion of length to diameter, the number of coils surrounding it, the conducting power of the metal of which the coils are formed, the size of the keeper or iron in which magnetism is induced, the degree of constancy of the battery, &c., complicate the experiments.

The most trustworthy general relation which has been ascertained is, that the magnetic attraction is as the square of the electric force; a result due to the researches of Lenz and Jacobi, and also of Sir W. S. Harris.

Lastly, electricity produces *chemical affinity*; and by its agency we are enabled to obtain effects of analysis or synthesis with which ordinary chemistry does not furnish us. Of these effects we have examples in the brilliant discoveries, by Davy, of the alkaline metals, and in the peculiar crystalline compounds made known by Crosse and Becquerel; though the latter are probably not always the definite result of the electric current, but are formed by the chemical changes produced by its bringing the molecules of the substances experimented on within the range of aggregation.

LIGHT.

In entering on the subject of Light, it will be well to describe briefly, and in a manner, as far as may be, independent of theory, the effects to which the term polarisation has been applied.

When light is reflected from the surface of water, glass, or many other media, it undergoes a change which disables it from being again similarly reflected in a direction at right angles to that at which it has been originally reflected. Light so affected is said to be polarised; it will always be capable of being reflected in planes parallel to the plane in which it has been first reflected, but incapable of being reflected in planes at right angles to that plane. At planes having a direction intermediate between the original plane of reflection, and a plane at right angles to it, the light will be capable of being partially reflected, and more or less so according as the direction of the second plane of reflection is more or less coincident with the original plane. Light, again, when passed through a crystal of Iceland spar, is what is termed doubly refracted, *i.e.* split into two divisions or beams, each having half the luminosity of the original incident light; each of these beams is polarised in planes at right angles to each other; and if they be intercepted by the mineral tourmaline, one of them is absorbed, so that only one polarised beam emerges. Similar effects may be produced by certain other reflections or refractions. A ray of light once polarised in a certain plane continues so affected throughout its whole subsequent course; and at any indefinite distance from the point where it originally underwent the change, the direction of the plane will be the same, provided the media through which it is transmitted be air, water, or certain other transparent substances which need not be enumerated. If, however, the

polarised ray, instead of passing through water, be made to pass through oil of turpentine, the definite direction in which it is polarised will be found to be changed ; and the change of direction will be greater according to the length of the column of interposed liquid. Instead of being an uniform plane, it will have a curvilinear direction, similar to that which a strip of card would have if forced along two opposite grooves of a rifle-barrel. This curious effect is produced in different degrees by different media. The direction also varies ; the rotation, as it is termed, of the plane being sometimes to the right hand and sometimes to the left, according to the peculiar molecular character of the medium through which the polarised ray is transmitted. These effects will be presently reverted to.

Light is, perhaps, that mode of force the reciprocal relations of which with the others have been the least traced out. Until the discoveries of Niepce, Daguerre, and Talbot, very little could be definitely predicated of the action of light in producing other modes of force. Certain chemical compounds, among which stand pre-eminent the salts of silver, have the property of suffering decomposition when exposed to light. If, for instance, recently formed chloride of silver be submitted to luminous rays, a partial decomposition ensues ; the chlorine is separated and expelled by the action of light, and the silver is precipitated. By this decomposition the colour of the substance changes from white to purple. If now, paper be impregnated with chloride of silver, which can be done by a simple chemical process, then partially covered with an opaque substance, a leaf for example, and exposed to a strong light, the chloride will be decomposed in all those parts of the paper where the light is *not* intercepted, and we shall have, by the action of light, a white image of the leaf on a purple ground. If similar paper be placed in the focus of a lens in a camera-obscura, the objects there depicted will decompose the chloride, just in the proportion in which they are luminous, and thus, as the most luminous parts of the image will most darken the chloride, we shall have a picture of the objects with reversed lights and shadows. The picture thus produced would not be permanent, as subsequent exposure would darken the light portion of the picture : to fix it, the paper

must be immersed in a solution which has the property of dissolving chloride of silver, but not metallic silver. Iodide of potassium will effect this; and the paper being washed and dried will then preserve a permanent image of the depicted objects. This was the first and simple process of Mr. Talbot; but it is defective as to the purposes aimed at, in many points. First, it is not sufficiently sensitive, requiring a strong light and a long time to produce an image; secondly, the lights and shadows are reversed; and thirdly, the coarse structure of the finest paper does not admit of the delicate traces of objects being distinctly impressed. These defects have been to a great extent remedied by a process subsequently discovered by Mr. Talbot, and which bears his name, and which has led to the collodion process, and others unnecessary to be detailed here.

The photographs of M. Daguerre, with which all are now familiar, are produced by holding a plate of highly-polished silver over iodine. A thin film of iodide of silver is thus formed on the surface of the metal; and when these iodized plates are exposed in the camera, a chemical alteration takes place. The portions of the plate on which the light has impinged, part with some of the iodine, or are otherwise changed —for the theory is somewhat doubtful—so as to be capable of ready amalgamation. When, therefore, the plate is placed over the vapour of heated mercury, the mercury attaches itself to the portions affected by light, and gives them a white frosted appearance; the intermediate tints are less affected, and those parts where no light has fallen, by retaining their original polish, appear dark: the iodide of silver is then washed off by hyposulphite of soda, which has the property of dissolving it, and there remains a picture in which the lights and shadows are as in nature, and the molecular uniformity of the metallic surface enables the most microscopic details to be depicted with perfect accuracy. By using chloride of iodine, or bromide of iodine, instead of iodine, the equilibrium of chemical forces is rendered still more unstable, so that images may be taken in an indefinitely short period—a period practically instantaneous.

It would be foreign to the object of this essay to enter upon

the many beautiful details into which the science of photography has branched out, and the many valuable discoveries and practical applications to which it has led. The short statement I have given above is perhaps superfluous, as, though the effects were new and surprising at the period when these Lectures were delivered, photographic processes have now become familiar, not only to the cultivator of science, but to the artist and amateur: the important point for consideration here is that light will chemically or molecularly affect matter. Not only will the particular compounds above selected as instances be changed by the action of light; but a vast number of substances, both elementary and compound, are notably affected by this agent, even those apparently the most unalterable in character, such as metals: so numerous, indeed, are the substances affected, that it has been supposed, not without reason, that matter of every description is altered by exposure to light.

The permanent impression stamped on the molecules of matter by light can be made to repeat itself by the same agency, but always with decreasing force. Thus a photograph placed opposite a camera containing a sensitive plate will be reproduced, but if the size of the image be equal to the picture, the second picture will be fainter in delineation than the first, and so on. Thus again, a photograph taken on a dull day cannot, by being placed in bright sunshine, be made to reproduce a second photograph of the same size and more distinctly marked than itself; I at least have never succeeded in such reproduction, and I am not aware that others have: the image loses in intensity as light itself does by each transmission. The surface of the metal or paper may give a brighter image from its being exposed to a more intense light, but the photographic details are limited to the intensity of the first impression, or rather to something short of this. A question of theoretical interest arises from the consideration of these reproduced photographs. We know that the luminosity of the image at the focus of a telescope is limited by the area of the object-glass. The image of any given object cannot be intensified by throwing upon it extraneous light; it is indeed diminished in intensity, and when for certain purposes astronomers illuminate the fields of their telescopes, they are

obliged to be contented with a loss of intensity in the telescopic image.

Now, let us suppose that the minutest details in the image of an object seen in a given telescope, and with a given power, are noted ; that then a photographic plate is placed in the focus of the same telescope so as to obtain a permanent impression of the image which has been viewed by the eye-glass. Could the observer, by throwing a beam of condensed light upon the photograph, enable himself to bring out fresh details ? or, in other words, could he use with advantage a higher power applied to the illuminated photograph than that which he used for direct vision ?

It is, perhaps, hardly safe to answer *à priori* this question; but the experiment of reproducing photographs would seem to show that more detail than that produced by the initial light cannot be obtained, and that we cannot expect to increase telescopic power by photography, though we may render observations more convenient, may by its means fix images seen on rare and favourable occasions, and may preserve permanent and infallible records of the state of astronomical objects at the periods of observation.

The effect of light on chemical compounds affords us a striking instance of the extent to which a force, ever active, may be ignored through successive ages of philosophy. If we suppose the walls of a large room covered with photographic apparatus, the small amount of light reflected from the face of a person situated in its centre would simultaneously imprint his portrait on a multitude of recipient surfaces. Were the cameras absent, but the room coated with photographic paper, a change would equally take place in every portion of it, though not a reproduction of form and figure. As other substances not commonly called photographic are known to be affected by light, the list of which might be indefinitely extended, it becomes a curious object of contemplation to consider how far light is daily operating changes in ponderable matter—how far a force, for a long time recognised only in its visual effects, may be constantly producing changes in the earth and atmosphere, in addition to the changes it produces in organised structures, which are now beginning to be exten-

sively studied. Every portion of light may be supposed to write its own history by a change more or less permanent in ponderable matter.

The late Mr. George Stephenson had a favourite idea, which would now be recognised as more philosophical than it was in his day, viz. that the light, which we nightly obtain from coal or other fuel, was a reproduction of that which had at one time been absorbed by vegetable structures from the sun. In one sense we now know this to be true; the rays of the sun enable the leaves of plants to decompose carbonic acid gas, absorbing and assimilating the carbon, and allowing the oxygen to escape. Mr. Balfour Stewart supposes that the want of photographic power in rays reflected from the leaves of growing plants is due to the energy of the rays having been used up in decomposing the carbonic acid. The carbon thus segregated by the sun's rays is ready to give out heat and light, whenever it may be recombined with oxygen by combustion. The conviction that the transient gleam leaves its permanent impress on the world's history, also leads the mind to ponder over the many possible agencies of which we of the present day may be as ignorant as the ancients were of the chemical action of light.

I have used the term light, and affected by light, in speaking of photographic effects; but, though the phenomena derived their name from light, it has been doubted by many competent investigators whether the phenomena of photography are not mainly dependent upon a separate agent accompanying light, rather than upon light itself. It is, indeed, difficult not to believe that a picture, taken in the focus of a camera-obscura, and which represents to the eye all the gradations of light and shade shown by the original luminous image, is not an effect of light; certain it is, however, that the different coloured rays exercise different actions upon various chemical compounds, and that the effects on many, perhaps on all of them, are not proportionate in intensity to the effects upon the visual organs. Those effects, however, appear to be more of degree than of specific difference; and, without pronouncing myself positively upon the question, hitherto so little examined, I think we may, at all

events in some cases, regard the action on photographic compounds as resulting from a function of light. So viewing it, we get light as an initiating force, capable of producing, mediately or immediately, the other modes of force. Thus, it immediately produces chemical action; and having this, we at once acquire a means of producing the others. At my Lectures in 1843, I showed an experiment by which the production of all the other modes of force by light is exhibited: I may here shortly describe it. A prepared daguerreotype plate is enclosed in a box filled with water, having a glass front with a shutter over it. Between this glass and the plate is a gridiron of silver wire; the plate is connected metallically with one extremity of a galvanometer coil, and the gridiron of wire with one extremity of a Breguet's helix—an elegant instrument formed by a coil of two metals, the unequal expansion of which indicates slight changes in temperature—the other extremities of the galvanometer and helix are connected by a wire, and the needles brought to zero. As soon as a beam of either daylight or the oxyhydrogen light is, by raising the shutter, permitted to impinge upon the plate, the needles are deflected. Thus, light being the initiating force, we get *chemical action* on the plate, *electricity* circulating through the wires, *magnetism* in the coil, *heat* in the helix, and *motion* in the needles.

If two plates of platinum be placed in acidulated water, and connected with a delicate galvanometer, the needle of this is always deflected, a result due to films of gas or other matter on the surface of the platinum, which no cleaning can remove. If, after the needle has returned to zero, which will not be the case for some hours or even days, one of the platinum surfaces be exposed to light, a fresh deflection of the needle takes place, due, as far as I have been able to resolve it, to an augmentation of the chemical action which had occasioned the original deflection, for the deviation is in the same direction. If, instead of white light, coloured light be permitted to impinge on the plate, the deviation is greater with blue than with red or yellow light, showing, in addition to other tests, that the effect is not due to the heat of the sun's rays, as the calorific effects of light are greater with red

than with blue light, while the chemical effects are the inverse.

There are other apparently more direct agencies of light in producing electricity and magnetism, such as those observed by Morichini and others, as well as its effects upon crystallisation; but these results have hitherto been of so indefinite a character, that they can only be regarded as presenting fields for experiment, and not as proving the relations of light to the other forces.

Light would seem directly to produce heat in the phenomena of what is termed absorption of light: in some of these we find that heat is developed in proportion to the disappearance of light. It was thought, and Franklin's experiment of different coloured cloths on snow exposed to the sun seemed to prove it, that heat was produced in a direct ratio to the absorption of light, but Dr. Tyndall has shown that this result does not depend on the colour, but on the chemical and physical character of the substance exposed, and that the heating effect is due mainly to non-luminous radiation. The heating powers of different colours are by no means in exact proportion to the intensity of their light as affecting the visual organs. Thus, red light when produced by refraction from a prism of glass, produces greater heating effect than yellow light in the phenomena of absorption, as has been observed by Sir W. Herschel. The red rays appear to produce a dynamic effect greater than any of the others; thus they penetrate water to a greater depth than the other colours; but, according to Dr. Seebeck, we get a further anomaly, viz. that when light is refracted by a prism of water the yellow rays produce the greater heating effect. The subject, therefore, requires much more experiment before we can ascertain the *rationale* of the action of the forces of light and heat in this class of phenomena.

In a former edition of this essay I suggested the following experiment on this subject:—Let a beam of light be passed through two plates of tourmaline, or similar substance, and the temperature of the second plate, or that on which the light last impinges, be examined by a delicate thermoscope, first when it is in a position to transmit the polarised beam coming

from the first plate, and secondly when it has been turned round through an arc of 90°, and the polarised beam is absorbed. I expected that, if the experiment were carefully performed, the temperature of the second plate would be more raised in the second case than in the first, and that it might afford interesting results when tried with light of different colours. I met with difficulties in procuring a suitable apparatus, and was endeavouring to overcome them, when I found that Knoblauch had, to some extent, realised this result. He finds that, when a solar beam, polarised in a certain plane, is transmitted perpendicularly to the axis of a crystal of brown quartz or tourmaline, the heat is transmitted in a smaller proportion than when the beam passes along the direction of the axis of the crystal.

It is generally—as far as I am aware, universally—true that, while light continues as light, even though reflected or transmitted by different media, little or no heat is developed; and, as far as we can judge, it would appear that, if a medium were perfectly transparent, or if a surface perfectly reflected light, not the slightest heating effect would take place; but, wherever light is absorbed, then heat takes its place, affording us, apparently, an instance of the conversion of light into heat, and of the fact that the force of light is not, in fact, annihilated, but merely changed in character, becoming, in this instance, converted into heat by impinging on solid matter, as in the instance mentioned in treating of heat, this force was shown to be converted into light by impinging on solid matter. But the different effects of different substances in transmitting and reflecting heat and light make this difficult of proof. One experiment, indeed, of Melloni, already mentioned, would seem to show that light may exist in a condition in which it does not produce heat, which our instruments are able to detect; but some doubt has recently been thrown on the accuracy of this experiment; probably the substances themselves through which the light is transmitted would be found to have been heated.

The recipient body, or that upon which light impinges, seems to exercise nearly as important an influence on our perceptions of light as the emittent body, or that from which

the light first proceeds. The recent experiments of Sir John Herschel and Mr. Stokes show that radiant impulses, which, falling on certain bodies, give no effect of light, become luminous when falling on other bodies.

Thus, let ordinary solar light be refracted by a prism (the best material for which is quartz), and the spectrum received on a sheet of paper, or of white porcelain; looking on the paper, the eye detects no light beyond the extreme violet rays. If, therefore, an opaque body be interposed so as just to cut off the whole visible spectrum, the paper would be dark or invisible, with the exception of some slight illumination from light reflected from dust in the air and from surrounding bodies. Substitute for that portion of the paper which was beyond the visible spectrum a piece of glass tinged by the oxide of uranium, and the glass is perfectly visible; so with a bottle of sulphate of quinine, or of the juice of horse-chesnuts, or even paper soaked in these latter solutions. Other substances exhibit in different degrees this effect, which is termed fluorescence; and among the substances which have hitherto been considered perfectly analogous as to their appearance when illuminated, notable differences are discovered. Thus it appears that emanations which give no impression of light to the eye, when impinging on certain bodies, become luminous when impinging on others. We might imagine a room so constructed that such emanations alone are permitted to enter it, which would be dark or light according to the substance with which the walls were coated, though in full daylight the respective coatings of the walls would appear equally white; or, without altering the coating of the walls, the room exposed to one class of rays might be rendered dark by windows which would be transparent to another class.

If, instead of solar light, the electrical light be employed for similar experiments, an equally striking effect can actually be produced. A design, drawn on white paper with a solution of sulphate of quinine and tartaric acid, is invisible by ordinary light, but appears with beautiful distinctness when illuminated by the electric light. Thus, in pronouncing upon a luminous effect, regard must be had to the recipient as well

as to the emittent body. That which is, or becomes, light when it falls upon one body is not light when it falls upon another. Probably the retinæ of the eyes of different persons differ to some extent in a similar manner; and the same substance, illuminated by the same spectrum, may present different appearances to different persons, the spectrum appearing more elongated to the one than to the other, so that what is light to the one is darkness to the other. A dependence on the recipient body may also, to a great extent, be predicated of heat. Let two vessels of water, the contents of the one clear and transparent, of the other tinged by some colouring matter, be suspended in a summer's sun; in a very short time a notable difference of temperature will be observed, the coloured having become much hotter than the clear liquid. If the first vessel be placed at a considerable distance from the surface of the earth, and the second near the surface, the difference is still more considerable. Carrying on this experiment, and suspending the first over the top of a high mountain, and the second in a valley, we may obtain so great a difference of temperature, that animals whose organisation is suited for the one temperature could not live in the other, and yet both are exposed to the same luminous rays at the same time, and substantially at the same distance from the emittent body—the substance nearer the sun is, in fact, colder than the more remote. So, with regard to the medium transmitting the influence: a greenhouse may have its temperature, or the vegetable growth within it, considerably varied by changing the glass of which its roof is made.

These effects have an important bearing on certain cosmical questions which have lately been much discussed, and should induce the greatest caution in forming opinions on such subjects as light and heat on the sun's surface, the temperature of the planets, &c. The latter may depend as much upon their physical constitution as upon their distance from the sun. Indeed, the planet Mars gives us a highly probable argument for this; for, notwithstanding that it is half as far again from the sun as the earth is, the increase of the white tracts at its poles during its winter, and their diminution during its summer, show that the temperature of the surface

of this planet oscillates about that of the freezing-point of water, as do the analogous zones of our planet. It is true in this we assume that the substance thus changing its state is water; but considering the many close analogies of this planet with the earth, and the identity in appearance of these very effects with what takes place on the earth, it seems not an improbable assumption.

So it by no means necessarily follows, that because Venus is nearer to the sun than the earth, that planet is hotter than our globe. The force emitted by the sun may take a different character at the surface of each different planet, and require different organisms or senses for its appreciation. Myriads of organised beings may exist imperceptible to our vision, even if we were among them; and we might be equally imperceptible to them!

However vain it may be, in the present state of science, to speculate upon such existences, it is equally vain to assume identity or close approximations to our own forms in those beings which may people other worlds. Reasoning from analogy, or from final causation, if that be admitted, we may feel convinced that the gorgeous globes of the universe are not unpeopled deserts; but whether the denizens of other worlds are more or less powerful, more or less intelligent, whether they have attributes of a higher or lower class than ourselves, is at present an utterly hopeless guessing.

Specific gravity and intelligence have no necessary connection. On our own planet five senses, and a mean density equal to that of water, are not invariably associated with intellectual or moral greatness, and the many arguments which have been used to prove that suns and planets other than the earth are uninhabited, or not inhabited by intellectual beings, might, *mutatis mutandis*, equally be used by the denizens of a sun or planet to prove that this world was uninhabited.

Men are too apt, because they are men, because their existence is the one thing of all importance to themselves, to frame schemes of the universe as though it was formed for man alone: painted by an artist of the sun, a man might not

represent so prominent an object of creation as he does when represented by his own pencil.

It seems to me no extravagant thought that, if alcohol and hereditary disease do not destroy the human race, a time may come when, from what will be known of the characteristics of some of the planets, a reasonable hypothesis of the characters of their inhabitants may be framed.

At the beginning of the last century, if it had been stated that a man could, from inspection of a fragment of bone, pronounce a well-founded opinion on the form and habits of the animal of which it had been part, such a conjecture would have been deemed ridiculous.

Could not the doctrine of Cuvier be extended? To a man who had never seen a fish, it would at first have seemed impossible that an animal could live under water, immersion in which had always, to his observation, proved fatal to terrestrial creatures; but a more far-seeing philosopher might argue from analogy that, as each part of the terrestrial surface was peopled with different animals varying, according to their organic adaptation, from a centipede to a kangaroo, it might not be improbable that the depths of an ocean might be inhabited by a suitable sentient creature. He might be led, by the observation of analogies, to form some idea of what would be the most suitable organism for respiration and locomotion under water, and so arrive at a not unapt idea of a fish.

The same mode of reasoning might lead him on thus:—'I see that the greater part of the surface of the globe I live on has organic beings, which, by some process, have not only become adapted to great varieties of climate, but which inhabit earth, air, and water. I see that a certain solution of salt in water which would be fatal to a trout is necessary to a mackerel. I therefore cannot conclude that, because a planet has a more attenuated atmosphere, a greater degree of heat, or a less degree of attraction of gravitation, it is therefore uninhabited.' He would be led to conclude, from all the variety of organic forms which people the present surface, lakes, rivers, oceans, and atmosphere of this planet, that it is

more than probable that planets, not differing in their condition so much from this as salt water, in which lives a whale, does from the air in which an eagle disports himself, are not barren while this is densely thronged with suitable organisms; he would think it improbable that, by a singular accident, his lot should have been cast on the only planet which is crowded at the present time with living organisms, and whose history shows that it has been so crowded in the past, that some of its rocks consist of a congeries of organic remains. Would it not appear reasonable to deduce the probability that there are other worlds, not peopled by creatures identical with or very similar to those which struggle for each spot of this earth, but as varied from those and from each other as the nature of their surroundings may require; and thence, finding by more perfect acquaintance the inevitable connection of surrounding conditions with the character of the organisms capable of existing among them, might he not more or less approximately deduce, from knowledge of the conditions, the character of the organisms?

As by the relation of inorganic matter to light, heat, &c. we have now tolerably clear proofs of the substances of which the sun and fixed stars are composed, it seems only a more perfected knowledge that may give the more complex deductions which I venture, at the risk of being thought fanciful, to indicate as possible in the distant future.

Light was regarded, by what was termed the corpuscular theory, as being in itself a specific matter emanating from luminous bodies, and producing the effects of sensation by impinging on the retina. This theory gave way to the undulatory one, which is generally adopted in the present day, and which regards light as resulting from the undulation of a specific fluid to which the name of ether has been given, which hypothetic fluid is supposed to pervade the universe, and to permeate the pores of all bodies.

In a lecture delivered in January 1842, when I first publicly advanced the views advocated in this essay, I stated that it appeared to me more consistent with known facts to regard light as resulting from a vibration or motion of the molecules of matter itself, rather than from a specific ether pervading it;

just as sound is propagated by the vibrations of wood, or as waves are by water. I am not here speaking of the character of the vibrations of light, sound, or water, which are doubtless very different from each other, but am only comparing them so far as they illustrate the propagation of force by motion in the matter itself.

I was not aware, at the time that I first adopted the above, view, and brought it forward in my Lectures, that the celebrated Leonard Euler had published a somewhat similar theory ; and, though I suggested it without knowing that it had been previously advanced, I should have hesitated in reproducing it, had I not found that it was sanctioned by so eminent a man as Euler, who can hardly be supposed to have overlooked any irresistible argument against it—the more so in a matter so much controverted and discussed as the undulatory theory of light was in his time.

Although this theory has been considered defective by a philosopher of high repute, I cannot see the force of the arguments by which it has been assailed ; and therefore, for the present, though with diffidence, I adhere to it. The fact itself of the correlation of the different modes of force is to my mind a very cogent argument in favour of their being affections of the same matter ; and though electricity, magnetism, and heat might be viewed as produced by undulations of the same ether as that by means of which light is supposed to be produced, yet this hypothesis offers greater difficulties with regard to the other affections than with regard to light : many of these difficulties I have already alluded to when treating of electricity ; thus, conduction and non-conduction are not explained by it ; the transmission of electricity through long wires in preference to the air which surrounds them, and which must be at least equally pervaded by the ether, is irreconcilable with such an hypothesis. The phenomena exhibited by these forces afford, as I think, equally strong evidence with those of light, of ordinary matter acting from particle to particle, and having no action at sensible distances. I have already instanced the experiments of Faraday on electrical induction, showing it to be an action of contiguous particles, which are strongly in favour of this view, and many experi-

ments which I have made on the voltaic arc, some of which I have mentioned in this essay, are, to my mind, confirmatory of it.

If it be admitted that one of the so-called imponderables is a mode of motion, then the fact of its being able to produce the others, and be produced by them, renders it highly difficult to conceive some as molecular motions and others as fluids or undulations of an ether. To the main objection of Dr. Young, that all bodies would have the properties of solar phosphorus if light consisted in the undulations of ordinary matter, it may be answered that so many bodies have this property, and with so great variety in its duration, that *non constat* all may not have it, though some for a time so short that the eye cannot detect it. M. E. Becquerel has made many experiments which support this view; the fact of the phosphorescence by insolation of a large number of bodies, and the phenomena of fluorescence afford evidence of dense matter being thrown into a state of undulation, or at all events molecularly affected by the impact of light, and is therefore an argument in support of the view which I am advocating. Dr. Young admits that the phenomena of solar phosphorus appear to resemble greatly the sympathetic sounds of musical instruments, which are agitated by other sounds conveyed to them through the air, and I am not aware that he gives any explanation of these effects on the ethereal hypothesis.

Some curious experiments of M. Niepce de St. Victor seem also to present an analogy in luminous phenomena to sympathetic sounds. An engraving which has been kept for some days in the dark is half-covered by an opaque screen, and then exposed to the sun; it is then removed from the light, the screen taken away, and the engraving placed opposite, and at a short distance from, photographic paper; an inverted image of that portion of the engraving which has been exposed to the sun is produced on the photographic paper, while the part which had been covered by the screen is not reproduced. If the engraving, after exposure, is allowed to remain in contact with white paper for some hours, and the white paper is then placed upon photographic paper, a faint

image of the exposed portion of the engraving is reproduced: similar results are produced by mottled marble exposed to the sun; an invisible tracing on paper by a fluorescent body, sulphate of quinine, is, after insolation, reproduced on photographic paper, &c. Insolated paper retains the power of producing an impression for a very long period, if it is kept in an opaque tube hermetically closed.

It is right to observe that these effects are supposed by many to be due to chemical emanations proceeding from the substances exposed to the sun, and which are believed to have undergone some chemical change by this exposure; this would still be a molecular effect, but it is desirable to await further experiment before forming a decided opinion.

The analogies in the progression of sound and light are very numerous: each proceeds in straight lines, until interrupted; each is reflected in the same manner, the angles of incidence and reflexion being equal; each is alternately nullified and doubled in intensity by interference; each is capable of refraction when passing between media of different density: this last effect of sound, long ago theoretically determined, has been experimentally proved by Mr. Sondhauss, who constructed a lens of films of collodion, which, when filled with carbonic acid, enabled him to hear the ticking of a watch placed at one focus of the lens, the ear of the experimenter being at the opposite focus. The ticking was not heard when the watch was moved aside from the focal point, though it remained at an equal distance from the ear. An experiment of M. Dové seems, indeed, to show an effect of polarisation of sound.

The phenomena presented by heat, viewed according to the dynamic theory, cannot be explained by the motion of an imponderable ether, but involve the molecular actions of ordinary ponderable matter. The doctrine of propagation by undulations of ordinary matter is very generally admitted by those who support the dynamical theory of heat; but the analogies of the phenomena presented by heat and light are so close, that I cannot see how a theory applied to the one agent should not be applicable to the other. When heat is

transmitted, reflected, refracted, or polarised, can we view that as an affection of ordinary matter, and when the same effects take place with light, view the phenomena as produced by an imponderable ether, and by that alone? To account for the phenomena of radiant heat the existence of an ether has been considered as necessary as for those of light, but we find that even with radiant heat the medium through which it is transmitted exercises an all-important influence on its absorption and transmission. In the year 1848 I showed that the variation in the cooling effect of different gases in which an ignited platinum wire was immersed was very remarkable, and appeared to depend more on their chemical and molecular constitution than on their physical characteristics, such as density, &c. Dr. Tyndall, in an elaborate series of papers, has shown that there are remarkable differences in the degree of absorption and radiation of heat by gases and vapours. These results are, to my mind, strongly corroborative of the view which dispenses with the hypothetic ether, and regards the imponderables as modes of motion of matter in different states of attenuation or of aggregation.

An objection that immediately occurs to the mind in reference to the ethereal hypothesis of light is, that the most porous bodies are opaque; cork, charcoal, pumice-stone, dried and moist wood, &c., all very porous and very light, are all opaque. This objection is not so superficial as it might seem at first sight. The theory which assumes that light is an undulation of an ethereal medium pervading gross matter, assumes the distances between the molecules or atoms of matter to be very great. Matter has been likened by Democritus, and by many modern philosophers, to the starry firmament, in which, though the individual monads are at immense distances from each other, yet they have in the aggregate a character of unity, and are firmly held by attraction in their respective positions and at definite distances. Now, if matter be built up of separate molecules, then, as far as our knowledge extends, the lightest bodies would be those in which the molecules are at the greatest distances, and those in which any undulation of a pervading medium would be the least interfered with by the separated particles—such bodies should

consequently be the most transparent. It may be argued that the size and want of continuity of the particles in porous bodies arrests or disperses the undulations, but this argument would apply on the ethereal hypothesis to all bodies.

If the analogy of the starry firmament held good, in this case an undulation or wave proportioned to the individual monads would be broken up by the number of them, and the very appearance of continuity which results, as in the Milky Way, from each point of vision being occupied by one of the monads, would show that at some portion of its progress the wave is interrupted by one of them, so that the whole may be viewed in some respect as a sheet of ordinary matter interposed in the ethereal expanse.

Even then, if it be admitted that a highly elastic medium pervades the interspaces, the separate masses as a whole must exercise an important influence on the progress of the wave.

Sound or vibrations of air meeting with a screen, or, as it were, sponge of diffused particles, would be broken up and dispersed by them; but if they be sufficiently continuous to take up the vibration and propagate it themselves, the sound continues comparatively unimpaired.

With regard, however, to liquid and gaseous bodies, there are very great difficulties in viewing them as consisting of separate and distant molecules. If, for instance, we assume with Young that the particles in water are comparatively as distant from each other as 100 men would be if dispersed at equal distances over the surface of England, the distance of these particles, when the water is expanded into steam, would be increased more than forty times, so that the 100 men would be reduced to two, and by further increasing the temperature this distance may be indefinitely increased; adding to the effects of temperature, rarefaction by the air-pump, we may again increase the distance, so that, if we assume any original distance, we ought, by expansion, to increase it to a point at which the distance between molecule and molecule should become measurable. But no extent of rarefaction, whether by heat or the air-pump, or both, makes the slightest change in the apparent continuity of matter; and gases, I find, retain

their peculiar character, as far as a judgment can be formed from their effect on the electric spark, throughout any extent of rarefaction which can experimentally be applied to them: thus the electric spark in protoxide of nitrogen, however attenuated, presents a crimson tint, that in carbonic oxide a greenish tint.

Without, however, entering on the metaphysical enquiry as to the constitution of matter (or whether the atomic philosophers or the followers of Boscovich are right), a problem which probably human appliances will never solve; and even admitting that an ethereal medium, not absolutely imponderable, as asserted by many, but of extreme tenuity, pervades matter, still ordinary or non-ethereal matter itself must exercise a most important action upon the transmission of light; and Dr. Young, who opposed the theory of Euler, that light was transmitted by undulations of gross matter itself, as sound is, was afterwards obliged to call to his assistance the vibrations of the ponderable matter of the refracting media, to explain why rays of all colours were not equally refracted, and other difficulties. One of his arguments in support of the existence of a permeating ether was, 'that a medium resembling in many properties that which has been denominated Ether does exist, is undeniably proved by the phenomena of electricity.' This seems to me, if I may venture to say so, of anything proceeding from so eminent a man, scarcely logical: it is supporting one hypothesis by another, and considering that to be proved which its most strenuous advocates admit to be surrounded by very many difficulties.

If it be said that there is not sufficient elasticity in ordinary matter for the transmission of undulations with such velocity as light is known to travel, this may be so if the vibrations be supposed exactly analogous to those of sound; but that molecular motion *can* travel with equal and even greater velocity than light, is shown by the rapidity with which electricity traverses a metal wire, where each particle of metal is undoubtedly affected. It has, moreover, been shown by the experiments of Mr. Latimer Clarke upon a length of wire of 760 miles, that whatever be the intensity of electrical currents, they are propagated with the same velocity, provided the

effects of lateral induction be the same—a striking analogy with one of the effects observed in the propagation of light and sound. The effects observed by MM. Fizeau and Foucault, of the slower progression of light in proportion as the transmitting medium is more dense, seem to me in favour of the view here advocated; as a greater degree of heat would be produced by light in proportion to the density of the medium, force would be thus carried off, and the molecular system disturbed, so that the progress of the motion should be more slow; but so many considerations enter into this question, and the phenomena are so extremely complex, that it would be rash to hazard any positive opinion.

Dr. Young ultimately came to the conclusion that it was simplest to consider the ethereal medium, together with the material atoms of the substance, as constituting together a compound medium denser than pure ether, but not more elastic. Ether, or highly attenuated matter, might thus be viewed as performing the functions which oil does with tracing-paper, giving continuity to the particles of gross matter, and in the interplanetary spaces forming itself the medium which transmits the undulations.

Since the period when Huyghens, Euler, and Young, the fathers of the undulatory theory, applied their great minds to this subject, a mass of experimental data has accumulated, all tending to establish the propositions, that whenever matter transmitting or reflecting light undergoes a structural change, the light itself is affected, and that there is a connection or parallelism between the change in the matter and the change in the affection of light, and conversely that light will modify or change the structure of matter and impress its molecules with new characteristics.

Transparency, opacity, refraction, reflection, and colour were phenomena known to the ancients, but sufficient attention does not appear to have been paid by them to the molecular states of the bodies producing these effects; thus the transparency or opacity of a body appears to depend entirely upon its molecular arrangement. If striæ occur in a lens or glass through which objects are viewed, the objects are distorted: increase the number of these striæ, the distortion is

so increased that the objects become invisible, and the glass ceases to be transparent, though remaining translucent; but alter completely the molecular structure, as by slow solidification, and it becomes opaque. Take, again, an example of a liquid and a gas: a solution of soap is transparent, air is transparent, but agitate them together so as to form a froth or lather, and this, though consisting of two transparent bodies, is opaque: the light being dashed about by minute reflections and refractions in different directions. From the same cause the reflection of light from the surface of these bodies, when so intermixed, is strikingly different from its reflection before mixture, in the one case giving to the eye a mere general effect of whiteness, in the other the images of objects in their proper shapes and colours.

To take a more refined instance: nitrogen is perfectly colourless, oxygen is perfectly colourless, but chemically united in certain proportions they form nitrous acid, a gas which has a deep orange-brown colour. I know not how the colour of this gas, or of such gases as chlorine or vapour of iodine, can be accounted for by the ethereal hypothesis, without calling in aid molecular affections of the matter of these gases.

Colour in many instances depends upon the thickness of the plate or film of transparent matter upon which light is incident; as in all those cases which are termed the colours of thin plates, of which the soap-bubble affords a beautiful instance.

When we arrive at the more recent discoveries of double refraction and polarisation, the effects of light are found to trace out as it were the structure of the matter affected, and the crystalline form of a body can be determined by the effects which a minute portion of it exercises on a ray of light.

Let a piece of good glass be placed in what is called a polariscope (*i.e.* an instrument in which light that has undergone polarisation is transmitted through the substance to be examined, and the emergent light is afterwards submitted to another substance capable of polarising light, or, as it is termed, an analyser), no change in effect will be observed. Remove the glass, heat it and suddenly or quickly cool it so

as to render it unannealed, in which state its molecules are in a state of tension or strain and the glass highly brittle, and on replacing it in the polariscope, a beautiful series of colours is perceptible. Instead of subjecting the glass to heat and sudden cooling, let it be bent or strained by mechanical pressure, and the colours will be equally visible, modified according to the direction of the flexure, and indicating by their course the curves where the molecular state has been changed by pressure. So I have found that if tough glue be elongated and allowed to cool in a stretched state, it doubly refracts light, and the colours are shown as in the instance of glass.

Submit a series of crystals to the same examination, and different figures will be formed by different crystals, bearing a constant and definite relation to the structure of the particular crystal examined, and to the direction in which, with reference to crystalline form, the ray crosses the crystal.

In the crystallised salts of paratartaric acid, M. Pasteur noticed two sets of crystals which were hemihedral in opposite directions, *i.e.* the crystals of one set were to those of the other as to their own image reflected in a mirror; on making a separate solution of each of these classes of crystals, he found that the solution of the one class rotated the plane of polarisation to the right, while that of the other class rotated it to the left, and that a mixture in proper proportions of the two solutions produced no deviation in the plane of polarisation. Yet all these three solutions are what is termed isomeric, that is, have as far as can be discovered the same chemical constitution.

In the above, and in innumerable other cases, it is seen that an alteration in the structure of a transparent substance alters the character and effects of the transmitted light. The phenomena of photography prove that light alters the structure of matter submitted to it; with regard even to vision itself, the persistence of images on the retina of the eye would seem to show that its structure is changed by the impact of light, the luminous impressions being as it were branded on the retina, and the memory of the vision being the scar of such brand. The science of photography has reference mainly to solid substances, yet there are many instances of liquid and gaseous bodies being changed by the action of light: thus hydrocyanic

acid, a liquid, undergoes a chemical change and deposits a solid carbonaceous compound by the action of light. Chlorine and hydrogen gases, when mixed and preserved in darkness, do not unite, but when exposed to light rapidly combine, forming hydrochloric acid.

The above facts—and many others might have been adduced—go far to connect light with motion of ordinary matter, and to show that many of the evidences which our senses receive of the existence of light result from changes in matter itself. When the matter is in the solid state, these changes are more or less permanent; when in the liquid or gaseous state, they are temporary in the greater number of instances, unless there be some chemical change effected, which is, as it were, seized upon during its occurrence, and a resulting compound formed, more stable than the original compound or mixture.

I might weary my reader with examples, showing that, in every case which we can trace out, the effects of light are changed by any and every change of structure, and that light has a definite connection with the structure of the bodies affected by it. I cannot but think that it is a strong assumption to regard ether, a purely hypothetical creation, as changing its elasticity or density for each change of structure, and to regard it as penetrating the pores of bodies of whose porosity we have in many cases no proof; the which pores must, moreover, have a definite and peculiar communication, also assumed for the purpose of the theory.

Ether is a most convenient medium for hypothesis: thus, if to account for a given phenomenon the hypothesis requires that the ether be more elastic, it is said to be more elastic; if more dense, it is said to be more dense; if it be required by hypothesis to be less elastic, it is pronounced to be less elastic; and so on. The advocates of the ethereal hypothesis certainly have this advantage, that the ether, being hypothetical, can have its characters modified or changed without any possibility of disproof either of its existence or modifications.

It may be that the refined mathematical labours on light, as on electricity, have given an undue and adventitious strength to the hypotheses on which they are based. M.

Comte thus expresses himself on this subject:—'Mathematicians, too frequently taking the means for the end, have embarrassed natural philosophy with a crowd of analytical labours founded upon hypotheses extremely hazardous, or even upon conceptions purely visionary, and consequently sober-minded people can see in them nothing more than simple mathematical exercises, of which the abstract value is sometimes very striking, without their influence in the slightest degree accelerating the natural progress of physics.'

An objection to which the view I have been advocating is open, and at first sight a formidable one, is the necessity involved in it of an universal plenum; for if light, heat, electricity, &c., be affections of ordinary matter, then matter must be supposed to be everywhere where these phenomena are apparent, and consequently there can be no vacuum.

These forces are transmitted through what are called vacua, or through interplanetary spaces, where matter, if it exist, must be in a highly attenuated state.

It may be safely stated that hitherto all attempts at procuring a perfect vacuum have failed. The ordinary air-pump gives us only highly rarefied air; and, by the principle of construction, even of the best, the operation depends upon the indefinite expansion of the volume of air in the receiver; even in the vacuum which is formed in this, so great is the tendency of matter to fill up space, that I have observed distilled water contained in a vessel within the exhausted receiver of a good air-pump has a taste of tallow, derived from the grease, or an essential oil contained in it, which is used to form an air-tight junction between the edges of the receiver and the pump-plate.

The Torricellian vacuum, or that of the ordinary barometer, is filled with the vapour of mercury; but it might be worth the trouble to ascertain what would be the effect of a good Torricellian vacuum, when the mercury in the tube is frozen, which might, without much difficulty, be now effected by the use of solid carbonic acid and ether; the only probable difficulty would be the different rates of contraction of mercury and glass, at such a degree of cold, and more particularly the contraction of mercury at the period of its solidification. Davy, indeed, endeavoured to form a vacuum, in a

somewhat similar manner, over fused tin, with but partial success; he also made many other attempts to obtain a perfect vacuum; his main object being to ascertain what would be the effect of electricity across empty space: he admits that he could not succeed in procuring a vacuum, but found electricity much less readily conducted or transmitted by the best vacuum he could procure than by the ordinary Boylean vacuum.

Morgan found no conduction by a good Torricellian vacuum; and, although Davy does not seem to place much reliance on Morgan's experiments, there was one point in which they were less liable to error than those of Davy. Morgan, whose experiments seem to have been carefully conducted, operated with hermetically-sealed glass tubes and by induced electricity, while Davy sealed a platinum wire into the extremity of the tube in which he sought to produce a vacuum. I have found in very numerous experiments which I made to exclude air from water, that platinum wires, most carefully sealed into glass, allow liquids to pass between them and the glass; and this gives some reason to believe that gases may equally pass through; indeed, I have observed such effect in the gas battery when it has been in action for a long period. Davy supposed that the particles of bodies may be detached, and so produce electrical effects in a vacuum; and such effects would more readily take place in his experiments, where a wire projected into the exhausted space, than in Morgan's, where the induced electricity was diffused over the surface of the glass.

M. Masson found that the barometric vacuum does not conduct a current of electricity, or even a discharge, unless the tension is considerable and sufficient to detach particles from the electrodes; and by adopting a plan of Dr. Andrews, viz. absorbing carbonic acid by potash, Mr. Gassiot has succeeded in forming vacua across which the powerful discharge from the Rhumkorf coil will not pass.

The limited but somewhat varying height at which the Aurora Borealis appears (according to such parallactic measurements as can be made), is not improbably due to the transmission of electricity, between the polar and equatorial regions of the atmosphere, being only effected at a certain

degree of rarefaction, the atmosphere below the observed height being too dense and that above too rare to allow the electricity to pass.

The odour which many metals, such as iron, tin, and zinc, emit, and the so-called thermographic radiations, can hardly be explained upon any other theory than the evaporation of an infinitesimally small portion of the metal itself.

So universal is the tendency of matter to diffuse itself into space, that it gave rise to the old saying that nature abhors a vacuum; an aphorism which, though cavilled at and ridiculed by the self-sufficiency of some modern philosophers, contains in a terse though somewhat metaphorical form of expression, a comprehensive truth, and evinces a large extent of observation in those who, with few of the advantages which we possess, first generalised by this sentence the facts of which they had become cognisant.

It has been argued that if matter were capable of infinite divisibility, the earth's atmosphere would have no limit, and that consequently portions of it would exist at points of space where the attraction of the sun and planets would be greater than that of the earth, and whence it would fly off to those bodies and form atmospheres around them. This was supposed to be negatived by the argument of the well-known paper of Dr. Wollaston; in which, from the absence of notable refraction near the margin of the sun and of the planet Jupiter, he considered himself entitled to conclude that the expansion of the earth's atmosphere had a definite limit, and was balanced at a certain point by gravitation: this deduction has been shown to be inconclusive by Dr. Whewell, and has also been impugned upon other grounds by Dr. Wilson. There is a point not adverted to in these papers, and which Wollaston does not seem to have considered, viz. that there is no evidence that the apparent discs of the sun and of Jupiter show us their real discs or bodies. Sir W. Herschel regards the margin of the visible discs as that of clouds or a peculiar state of atmosphere, and the rapidly changing character of the apparent surfaces renders some such conclusion necessary. If this be so, refraction of an occulted star could not be detected —at all events in the denser portion of the atmosphere.

Sir W. Herschel's observations go to prove that the sun and Jupiter have dense atmospheres, while Wollaston's were believed to prove that they have no appreciable atmospheres. The results of recent observations with the spectroscope are in favour of the former conclusion; the chromosphere being found to extend far beyond the defined disc of the sun.

If it be admitted, or considered proved, that the sun and planets have atmospheres—and little doubt now exists on this point—then the grounds upon which Wollaston founded his arguments are untenable; and there appears no reason why the atmosphere of the different planets should not be, with reference to each other, in a state of equilibrium. Ether, which term we may apply to the highly-attenuated matter existing in the interplanetary spaces, being an expansion of some or all of these atmospheres, or of the more volatile portions of them, would thus furnish matter for the transmission of the modes of motion which we call light, heat, &c.; and possibly minute portions of these atmospheres may, by gradual accretions and subtractions, pass from planet to planet, forming a link of material communication between the distant monads of the universe.

The assumption of the universal presence of matter is common to the theory of the transmission of light by the undulations of ordinary matter and to the other two theories, which equally presuppose the non-existence of a vacuum; for, according to the emissive or corpuscular theory, the vacuum is filled by the matter itself, of light, heat, &c.; according to the ethereal it is filled by the all-penetrating ether. Of the existence of matter in the interplanetary spaces we have some evidence in the diminishing periods of comets; and where, from its highly attenuated state, the character of the medium by which the forces are conveyed cannot be tested, the term ether is a very admissible generic name for such medium.

Newton has some curious passages on the subject-matter of light. In the 'Queries to the Optics' he says:—

'Are not gross bodies and light convertible into one another, and may not bodies receive much of their activity from the particles of light which enter their composition? . . . The changing of bodies into light and light into

bodies is very conformable to the course of Nature, which seems delighted with transmutations. Water, which is a very fluid, tasteless salt, she changes by heat into vapour, which is a sort of air, and by cold into ice, which is a hard, pellucid, brittle, fusible stone, and this stone returns into water by heat, and vapour returns into water by cold. . . . And, among such various and strange transmutations, why may not Nature change bodies into light, and light into bodies?'

Newton has here seemingly in his mind the emissive theory of light; but the passages might be applied to either theory; the analogy he saw in the change of state of matter, as in ice, water, and vapour, with the hypothetic change into light, is very striking, and would seem to show that he regarded the change or transmutation of which he speaks as one analogous to the known changes of state, or consistence, in ordinary matter.

The difference between the view which I am advocating and that of the ethereal theory as generally enunciated is, that the matter which in the interplanetary spaces serves as the means of transmitting by its undulations light and heat, I should regard as possessing the properties of ordinary, or as it has sometimes been called gross matter, and particularly weight; though, from its extreme rarefaction, it would manifest these properties in an indefinitely small degree; whilst, near to or on the surface of the earth, that matter attains a density cognisable by our means of experiment, and the dense matter is itself, in great part, if not entirely, the conveyer of the undulations in which these agents consist. Doubtless, in very many of the forms which matter assumes it is porous, and pervaded by more volatile essences, which may differ as much in kind as matter does. In these cases a composite medium, such as that indicated by Dr. Young, would result; but even on such a supposition, the denser matter would probably exercise the more important influence on the undulations. Returning to the somewhat strained hypothesis, that the particles of dense matter in a so-called solid are as distant as the stars in heaven, still a certain depth or thickness of such solid would present at every point of space a particle or rock in the successive progress of a wave, which particles, to carry on the movement, must vibrate in unison with it.

At the utmost, our assumption, on the one hand, is, that wherever light, heat, &c., exist, ordinary matter exists, though it may be so attenuated that we cannot recognise it by the test of gravitation; and that to the expansibility of matter no limit can be assigned. On the other hand, a specific matter without weight must be assumed, of the existence of which there is no evidence but in the phenomena for the explanation of which its existence is supposed. To account for the phenomena the ether is assumed, and to prove the existence of the ether the phenomena are cited. For these reasons, and others above given, I think that the assumption of the universality of ordinary matter is the least gratuitous.

Οὐδέν τι τοῦ παντὸς κενὸν πέλει οὐδὲ περισσόν.

A question has often occurred to me and possibly to others: Is the continuance of a luminous impulse in the interplanetary spaces perpetual, or does it after a certain distance dissipate itself and become lost as light—I do not mean by mere divergence directly as the squares of the distances it travels, but does the physical impulse itself lose force as it proceeds? Upon the view I have advocated, and indeed upon any undulatory hypothesis, there must be some resistance to its progress; and unless the matter or ether in the interplanetary spaces be infinitely elastic, and there be no lateral action of a ray of light, there must be some loss. That it is exceedingly minute is proved by the distance light travels. Stars whose parallax is ascertained are at such a distance from the earth that their light, travelling at the rate of 192,500 miles in a second, takes more than ten years to reach the earth; so that we see them as they existed ten years ago. The distance of most visible stars is probably far greater than this, and yet their brilliance is great, and increases when their rays are collected by the telescope in proportion *ceteris paribus* to the area of the object-glass or speculum. There is, however, an argument of a somewhat speculative character, by which light would seem to be lost or transformed, into some other force in the interplanetary spaces.

Every increase of space-penetrating power in the telescope

gives us a new field of visible stars. If this expansion of the stellar universe go on indefinitely and no light be lost, then, assuming the fixed stars to be of an average equal brightness with our sun, and to fill up every point of space, and that no light be lost other than that by divergence, the night ought to be equally luminous with the day; for though the light from each point diminishes in intensity as the square of the distance, the number of luminous points would increase as the square of the distance, and thus occupy the whole visible space around us; and if every point of space is occupied by an equally brilliant point of light, the distance of the points from us becomes immaterial. The loss of light intercepted by stellar bodies would make no difference in the total quantity of light reaching us, for each of these would yield from its own self-luminosity at least as much light as it intercepted. Light may, however, be intercepted by non-luminous bodies, such as planets revolving round the self-luminous stars; but, making every allowance for these, it is difficult to understand why we get so little light at night from the stellar universe, without assuming that some light is lost in its progress through space—not lost absolutely, for that would be an annihilation of force—but converted into some other mode of motion.

It may be objected that this hypothesis assumes the stellar universe to be illimitable: if pushed to its extreme so as to make the light of night equal that of day, provided no stellar light be lost, it does make this assumption; but even this seems a far more rational assumption to make than that the stellar universe is limited. Our experience gives no indication of a limit; each improvement in telescopic power gives us new realms of stars or of nebulæ, which, if not stellar clusters, are at all events self-luminous matter; and if we assume a limit, what is it? We cannot conceive a physical boundary, for then immediately comes the question, what bounds the boundary? and to suppose the stellar universe to be bounded by infinite space or by infinite chaos, that is to say, to suppose a spot—for it would then become so—of matter in definite forms, with definite forces, and probably teeming with definite organic beings, plunged in a universe of nothing, is, to my

mind at least, far more unphilosophical than to suppose a boundless universe of matter existing in forms and actions more or less analogous to those which, as far as our examination goes, pervades space. But without speculating on topics in which the mind loses itself, it may not unreasonably be expected that a greater amount of light would reach us from the surrounding self-luminous spheres, were not some portion lost as light by its action on the medium which conveys the impulses. What force this becomes, or what it effects, it would be vain to speculate upon.

After advancing the above conjecture, I found that Struvé had been led, from astronomical observations, to the conclusion that some light is lost in the interplanetary spaces; he gives as an approximation one per cent. as lost by the passage of light from a star of the first magnitude, assuming a mean or average distance.

MAGNETISM.

MAGNETISM, as was proved by the important discovery of Faraday, will produce *electricity*, but with this condition, that being in itself static, to produce a dynamic force, motion must be superadded to it: without this it is, in fact, directive, not motive, altering the direction of other forces, but not, in strictness, initiating them. It is difficult to convey a definite notion of the force of magnetism, and of the mode in which it affects other forces. The following illustration may give a rude idea of magnetic polarity. Suppose a number of wind-vanes, say of the shape of arrows, with the spindles on which they revolve arranged in a row, but the vanes pointing in various directions: a wind blowing from the same point with an uniform velocity will instantly arrange these vanes in a definite direction, the arrow-heads or narrow parts pointing one way, the swallow-tails or broad parts another. If they be delicately suspended on their spindles, a very gentle breeze will so arrange them, and a very gentle breeze will again deflect them; or, if the wind cease, and they have been originally subject to other forces, such as gravity from unequal balancing, they will return to irregular positions, themselves creating a slight breeze by their return. Such a state of things will represent the state of the molecules of soft iron; electricity acting on them—not indeed in straight lines, but in a definite direction—produces a polar arrangement, which they will lose as soon as the dynamic inducing force is removed.

Let us now suppose the vanes, instead of turning easily, to be more stiffly fixed to the axes, so as to be turned with difficulty: it will require a stronger wind to move them and arrange them definitely; but when so arranged, they will retain their position; and should a gentle breeze spring up in

another direction, it will not alter their position, but will itself be definitely deflected. Should the conditions of force and stability be intermediate, both the breeze and the vanes will be slightly deflected; or, if there be no breeze, and the spindles be all moved in any direction, preserving their parallel relation, they will themselves create a breeze. Thus it is with the molecules of hard iron or steel in permanent magnets: they are polarised with greater difficulty than those of soft iron, but, when so polarised, they cannot be affected by a feeble current of electricity. Again, if the magnets be moved, they themselves originate a current of electricity; and, lastly, the magnetic polarity and the electric current may be both mutually affected, if the degrees of mobility and stability be intermediate

The above instance will, of course, be taken only as an approximation, and not as binding me to any closer analogy than is generally expected of a mechanical illustration. It is difficult to convey by words a definite idea of the dual or antithetic character of force involved in the term polarity. The illustration I have employed may, I hope, somewhat aid in elucidating the manner in which magnetism acts on the other dynamic forces; *i.e.*, definitely directing them, but not initiating them, except while in motion.

Magnets being moved in the direction of lines, joining their poles, produce electrical currents in such neighbouring bodies as are conductors of electricity, in directions transverse to the line of motion; and if the direction of motion or the position of the magnetic poles be reversed, the current of electricity flows in a reverse direction. So if the magnet be stationary, conducting bodies moved across any of the lines of magnetic force (*i.e.* lines in the direction of which the mutual action of the poles of the magnet would place minute portions of iron) have currents of electricity developed in them, the direction of which is dependent upon that of the motion of the substance with reference to the magnetic poles. Thus, as bodies affected by an electrical current are definitely moved by a magnet in proximity to them, so conversely bodies moved near a magnet have an electrical current developed in them. Magnetism can, then, through the medium of electri-

city, produce *heat*, *light*, and *chemical affinity*. *Motion* it can directly produce under the above conditions; *i.e.* a magnet being itself moved will move other ferreous bodies; these will acquire a static condition of equilibrium, and be again moved when the magnet is also moved. By motion or arrested motion only, could the phenomena of magnetism ever have become known to us. A magnet, however powerful, might rest for ever unnoticed and unknown, unless it were moved near to iron, or iron moved near to it, so as to come within the sphere of its attraction.

But even with other than either magnetic or electrified substances, all bodies will be moved when placed near the poles of very powerful magnets—some taking a position axially, or in the line from pole to pole of the magnet; others equatorially, or in a direction transverse to that line—the former being attracted, the latter apparently repelled, by the poles of the magnet. These effects, according to the views of Faraday, show a generic difference between the two classes of bodies, magnetics and diamagnetics; according to others, a difference of degree or a resultant of magnetic action; the less magnetic substance being forced into a transverse position by the magnetisation of the more magnetic medium which surrounds it.

According to the view given above, magnetism may be produced by the other forces, just as the vanes in the instance given are definitely deflected, but cannot produce them except when in motion: motion, therefore, is to be regarded in this case as the initiative force. Magnetism will, however, directly affect the other forces—light, heat, and chemical affinity, and change their direction or mode of action, or, at all events, will so affect matter subjected to these forces, that their direction is changed. Since these lectures were delivered, Faraday has discovered a remarkable effect of the magnetic force in occasioning the deflection of a ray of polarised light.

If a ray of polarised light pass through water, or through any transparent liquid or solid which does not alter or turn aside the plane of polarisation, and the column, say of water,

through which it passes be subjected to the action of a powerful magnet, the line of magnetic force, or that which would unite the poles of the magnet, being in the same direction as the ray of polarised light, the water acquires, with reference to the light, similar, though not quite identical, properties to oil of turpentine—the plane of polarisation is rotated, and the direction of this rotation is changed by changing the direction of the magnetic force: thus, if we suppose a polarised ray to pass first in its course the north pole of the magnet, then between that and the south pole, it will be deflected, or curved to the right; while if it meet the south pole first in its course, it will, in its journey between that and the north pole, be turned to the left. If the substance through which the ray is transmitted be of itself capable of deflecting the plane of polarisation, as, for instance, oil of turpentine, then the magnetic influence will increase or diminish this rotation, according to its direction. A similar effect to this is observed with polarised heat when the medium through which it is transmitted is subjected to magnetic influence.

Whether this effect of magnetism is rightly termed an effect upon light and heat, or is a molecular change of the matter transmitting the light and heat, is a question the resolution of which must be left to the future; at present, the answer to it would depend upon the theory we adopt. If the view of light and heat which I have stated be adopted, then we may fairly say that magnetism, in these experiments, directly affects the other forces; for light and heat being, according to that view, motions of ordinary matter, magnetism, in affecting these movements, affects the forces which occasion them. If, however, the other theories be adhered to, it would be more consistent with the facts to view these results as exhibiting an action upon the matter itself, and the heat and light as secondarily affected.

When substances are *undergoing* chemical changes, and a magnet is brought near them, the direction or lines of action of the chemical force will be changed. There are many old experiments which probably depended on this effect, but which were erroneously considered to prove that permanent magnetism could produce or increase chemical action: these

have been extended and explained by Mr. Hunt and Mr. Wartmann, and are now better understood.

The above cases are applicable to the subject of the present Essay, inasmuch as they show a relation to exist between magnetic and the other forces, which relation is, in all probability, reciprocal; but in these cases there is not a production of light, heat, or chemical affinity, by magnetism alone, but a change in their direction or mode of action.

There is, however, that which may be viewed as a dynamic condition of magnetism; *i.e.* its condition at the commencement and the termination, or during the increment or decrement of its development. While iron or steel is being rendered magnetic, and as it progresses from its non-magnetic to its maximum magnetic state, or recedes from its maximum to zero, it exhibits a dynamic force; the molecules are, it may be inferred, in motion. Similar effects can then be produced to those which are produced by a magnet whilst in motion.

An experiment which I published in 1845 tends, I think, to illustrate this, and in some degree to show the character of the motion impressed upon the molecules of a magnetic metal at the period of magnetisation. A tube filled with the liquid in which magnetic oxide of iron had been prepared, and terminated at each end by plates of glass, is surrounded by a coil of coated wire. To a spectator looking through this tube a flash of light is perceptible whenever the coil is electrised, and less light is transmitted when the electrical current ceases, showing a symmetrical arrangement of the minute particles of magnetic oxide while under the magnetic influence. In this experiment it should be borne in mind that the particles of oxide of iron are not shaped by the hand of man, as would be the case with iron filings, or similar minute portions of magnetic matter, but being chemically precipitated, are of the form given to them by nature.

While magnetism is in the state of change above described it will produce the other forces; but it may be said, while magnetism is thus progressive, some other force is acting on it, and therefore it does not initiate: this is true, but the same may be said of all the other forces; they have no commencement that we can trace. We must ever refer them back to

some antecedent force equal in amount to that produced, and therefore the word initiation cannot in strictness apply, but must only be taken as signifying the force selected as the first: this is another reason why the idea of abstract causation is inapplicable to physical production. To this point I shall again advert.

Electricity may thus be produced directly by magnetism, either when the magnet as a mass is in motion, or when its magnetism is commencing, increasing, decreasing, or ceasing; and heat may seemingly be directly produced by magnetism. Mr. Joule and Mr. Van Breda showed that iron was heated when magnetised or demagnetised, and I, not knowing of their experiments, took some pains to prove this, to eliminate courses of error and to show that the heating effect took place with other magnetic metals than iron. This was done first by subjecting a bar of iron, nickel, or cobalt to the influence of a powerful electro-magnet, which was rapidly magnetised and demagnetised in reverse directions, the electro-magnet itself being kept cool by cisterns of water, so that the magnetic metal subjected to the influence of magnetism was raised to a higher temperature than the electro-magnet itself, and could not, therefore, have acquired its increased temperature by conduction or radiation of heat from the electro-magnet; and secondly, by rotating a permanent steel magnet with its poles opposite to a bar of iron, a thermo-electric pile being placed opposite the latter.

It may, however, be fairly contended that in all these experiments the heating effect was the result of electrical currents developed by the magnet, and not directly by the magnetism.

Dr. Maggi covered a plate of homogeneous soft iron with a thin coating of wax mixed with oil; a tube traversed the centre, through which the vapour of boiling water was passed. The plate was made to rest on the poles of an electro-magnet, with card interposed. When the iron is not magnetised, the melted wax assumes a circular form, the tube occupying the centre, but when the electro-magnet is put in action, the curve marking the boundary of the melted substance changes its form and becomes elongated in a direction transverse to the

line joining the poles, showing apparently that the conducting power of the iron for heat is changed by magnetisation.

Thus we get heat produced by magnetism and the conduction of heat altered by it in a direction having a definite relation to the direction of the magnetism. Is it necessary to call in aid ether or the substance 'caloric' to explain these results? Is it not more rational to regard the calorific effects as changes in the molecular arrangements of the matter subjected to magnetism?

There is no obvious reason why magnetism, in the dynamic state, *i.e.* either when the magnet is in motion, or when the magnetic intensity is varying, may not also directly produce chemical affinity and light, though up to the present time such has not been proved to be the case; the reciprocal effect, also, of the direct production of magnetism by light or heat has not yet been experimentally established.

I have used, in contradistinction, the terms dynamic and static to represent the different states of magnetism. The applications I have made of these terms may be open to some exception, but I know of no other words which will so nearly express my meaning.

The static condition of magnetism resembles the static condition of other forces: such as the state of tension existing in the beam and cord of a balance, or in a charged Leyden phial. One of the old definitions of force was, that which caused change in motion; and yet even this definition presents a difficulty: in a case of static equilibrium, such, for instance, as that which obtains in the two arms of a balance, we get the idea of force without any palpable apparent motion: whether there be really an absence of motion may be a doubtful question, as such absence would involve in this case perfect elasticity, and, in all other cases, a stability which, in a long course of time, nature generally negatives, showing, as I believe, an inseparable connection of motion with matter, and an impossibility of a perfectly immobile or durable state. So with magnetism; I believe no magnet can exist in an absolutely stable state, though the duration of its stability will be proportionate to its original resistance to assuming a polarised condition. This, however, must be taken merely as a matter

of opinion: we have, in support of it, the general fact that magnets do deteriorate in the course of years; and we have the further general fact of the instability, or fluxional state, of all nature, when we have an opportunity of fairly investigating it at different and remote periods: in many cases, however, the action is so slow that the changes escape human observation, and, until this can be brought to bear over a proportionate period of time, the proposition cannot be said to be experimentally or inductively proved, but must be left to the mental conviction of those who examine it by the light of already acknowledged facts.

All cases of static force present the same difficulty: thus, two springs pressing against each other would be said to be exercising force; and yet there is no resulting action, no heat, no light, &c.

So if gas be compressed by a piston, at the time of compression heat is given off; but when this is abstracted, although the pressure continues, no further heat is eliminated. Thus, by an equilibrium produced by opposing forces, motion is locked up, or in abeyance, as it were, and may be again developed when the forces are relieved from the tension. But in the first instance, in producing the state of tension, force has to be employed; and as we have said in treating of mechanical force, so with the other forces the original change which disturbs equilibrium produces other changes which go on without end. Thus, by the act of charging a Leyden phial, the cylinder, the rubber, and the adjoining portions of the electrical machine have each and all their states changed, and thence produce changes in surrounding bodies *ad infinitum*; when the jar is discharged, converse changes are again produced.

The term potential energy has of late been much used to signify locked up force, and though I am far from denying its occasional utility, I am a little fearful of its extension; like other words, it has only a relative application; a heated globe has a potential energy capable of becoming active when brought within colder surroundings, but none if all around it be of the same temperature as itself. A piece of ice may be said to have a potential energy when brought into warmer surroundings. Oxygen has a potential energy when brought

into relation with hydrogen. Hydrogen has it when brought into relation with oxygen. Mixed oxygen and hydrogen have an enormous power, if ignited when the surrounding media are at a certain temperature, but if these are above the temperature at which decomposition of water by heat takes place, the potential energy of the gases in relation to each other is *nil*; they cannot combine by the application of heat, and they must be chilled to evolve active force.

Dr. Odling has shown that these and other substances on entering into combination necessarily evolve heat, and it seems probable that, if the heat surrounding them be greater than that evolved by their combination, the heat they would evolve cannot be conveyed away, and consequently that they would not combine.

A stone on a hill has a potential energy with reference to the valley beneath, and when it has reached that it has a potential energy with reference to the bottom of a mine.

Every particle of matter has in it a locked up or potential energy when it is in equilibrium, but active when the equilibrium is disturbed. It is capable as the case may be of heating, of cooling, of oxidating, of deoxidating, of evaporating, of condensing, &c., according to the state of surrounding bodies.

A weight raised from the earth not only has the potential energy of its tendency to fall, but, as I have before observed, changes the centre of gravity of the earth (which may be said to have thus acquired new potential energy), and alters its relation to the solar and other systems. The whole universe has thus acquired new potential energy when a stone has been lifted by a crane and placed on a wall. Could we not hope to arrive at more accurate scientific ideas if we expressed ourselves in terms of actual motion and discarded latent or non-acting force? Thus, if by raising a stone the centre of gravity of the earth be changed, the sun and planets, &c. are affected; so that when we produce what is called a state of potential energy, we are actually eliminating dynamic force from other bodies.

Were the state of things contemplated by Sir W. Thomson (p. 67) to be the ultimate result of all other energies being resolved into uniform diffused heat, the total energy in the

universe would be the same as at present, but it would really be the same result as though there were no energy at all, as none could act, all would be in a state of unchanging stable equilibrium 'and everything be nought.'

As with heat, light, and electricity, daily accumulating observations tend to show that each change in the phenomena to which these names are given is accompanied by a change either temporary or permanent in the matter affected by them; so many experiments on magnetism have connected magnetic phenomena with a molecular change in the subject-matter. In addition to the cases previously given the following may be mentioned out of many. M. Wertheim has shown that the elasticity of iron and steel is altered by magnetisation; the coefficient elasticity of in iron being temporarily, in steel permanently diminished.

He has also examined the effects of torsion upon magnetised iron, and concludes, from his experiments, that in a bar of iron arrived at a state of magnetic equilibrium, temporary torsion diminishes the magnetism, and that the untwisting or return to its primitive state restores the original degree of magnetisation.

M. Guillemin observed that a bar slightly curved by its own weight is straightened by being magnetised. Mr. Page and Mr. Marrion discovered that a sound is emitted when iron or steel is rapidly magnetised or demagnetised; and Mr. Joule found that a bar of iron is slightly elongated by magnetisation.

Again, with regard to diamagnetic bodies. M. Matteucci found that the mechanical compression of glass altered the rotatory power of magnetism upon a ray of polarised light which the glass transmitted. He further considered that a change took place in the temper of portions of glass which he submitted to the influence of powerful magnets.

The same arguments which have been submitted to the reader as to the other affections of matter being modes of molecular motion, are therefore equally applicable to magnetism.

CHEMICAL AFFINITY.

CHEMICAL AFFINITY, or the force by which dissimilar bodies tend to unite and form compounds differing generally in character from their constituents, is that mode of force of which the human mind has hitherto formed the least definite idea. The word itself—*affinity*—is ill-chosen, its meaning, in this instance, bearing no analogy to its ordinary sense; and the mode of its action is conveyed by certain conventional expressions, no dynamic theory of it worthy of attention having been adopted. Its action so modifies and alters the character of matter, that the changes it induces have acquired, not perhaps very logically, a generic contradistinction from other material changes, and we thus use, as contradistinguished, the terms physical and chemical.

The main distinction between chemical affinity and physical attraction or aggregation, is the difference of character of the chemical compound from its components. This is, however, but a vague line of demarcation; in many cases, which would be classed by all as chemical actions, the change of character is but slight; in others, as in the effects of neutralisation, the difference of character would be a result which would be expected to follow from physical attraction of dissimilar substances, the previous characters of the constituents depending upon this very attraction or affinity: thus an acid corrodes because it tends to unite with another body; when united, its corrosive power, *i.e.* its tendency to unite, being satiated, it cannot, so to speak, be further attracted, and it necessarily loses its corrosive power. But there are other cases where no such result could *à priori* be anticipated, as where the attraction or combining tendency of the compound is higher than that of any of its constituents; thus, who could, by physical

reasoning, anticipate a substance like nitric acid from the combination of nitrogen and oxygen?

The nearest approach, perhaps, that we can form to a comprehension of chemical action, is by regarding it (vaguely, perhaps) as a molecular attraction or motion. It will directly produce motion of definite masses, by the resultant of the molecular changes it induces: thus, the projectile effects of gunpowder may be cited as familiar instances of motion produced by chemical action. It may be a question whether, in this case, the force which occasions the motion of the mass is a conversion of the force of chemical affinity, or whether it is not, rather, a liberation of other forces existing in a state of static equilibrium, and having been brought into such state by previous chemical actions; but, at all events, through the medium of electricity chemical affinity may be directly and quantitatively converted into the other modes of force. By chemical affinity, then, we can directly produce electricity; this latter force was, indeed, said by Davy to be chemical affinity acting on masses: it appears rather to be chemical affinity acting in a definite direction through a chain of particles; but by no definition can the exact relation of chemical affinity and electricity be expressed; for the latter, however closely related to the former, yet exists where the former does not, as in a metallic wire, which, when electrified, or conducting electricity, is, nevertheless, not chemically altered, or, at least, not known to be chemically altered.

Volta, the antitype of Prometheus, first enabled us definitely to relate the forces of chemistry and electricity. When two dissimilar metals in contact are immersed in a liquid belonging to a certain class, and capable of acting chemically on one of them, what is termed a voltaic circuit is formed, and, by the chemical action, that peculiar mode of force called an electric current is generated, which circulates from metal to metal, across the liquid, and through the points of contact.

Let us take, as an instance of the conversion of chemical force into electrical, the following, which I made known some years ago. If gold be immersed in hydrochloric acid, no chemical action takes place. If gold be immersed in nitric

acid, no chemical action takes place; but mix the two acids, and the immersed gold is chemically attacked and dissolved: this is an ordinary chemical action, explained as the result of a double chemical affinity. In hydrochloric acid, which is composed of chlorine and hydrogen, the affinity of chlorine for gold being less than its affinity for hydrogen, no change takes place; but when the nitric acid is added, this latter containing a great quantity of oxygen in a state of feeble combination, the affinity of oxygen for hydrogen opposes that of hydrogen for chlorine, and then the affinity of the latter for gold is enabled to act, the gold combines with the chlorine, and chloride of gold remains in solution in the liquid. Now, in order to exhibit this chemical force in the form of electrical force, instead of mixing the liquids, place them in separate vessels or compartments, but so that they may be in contact, which may be effected by having a porous material, such as unglazed porcelain, amianthus, &c., between them. Immerse in each of these liquids a strip or wire of gold: as long as the pieces of gold remain separated, no chemical or electrical effect takes place; but the instant they are brought into metallic contact, either immediately or by connecting each with the same metallic wire, chemical action takes place—the gold in the hydrochloric acid is dissolved, electrical action also takes place, the nitric acid is deoxidised by the transferred hydrogen, and a current of electricity may be detected in the metals, or connecting metal, by the application of a galvanometer or any instrument appropriate for detecting such effect.

There are few, if any, chemical actions which cannot be experimentally made to produce electricity: the oxidation of metals, the burning of combustibles, the combination of oxygen and hydrogen, &c., may all be made sources of electricity. The common mode in which the electricity of the voltaic battery is generated is by the chemical action of the oxygen of water upon zinc; this action is increased by adding certain acids to the water, which enable it to act more powerfully upon the zinc, or in some cases act themselves upon it; and one of the most powerful chemical actions known—that of nitric acid upon oxidable metals—is that which produces the most

powerful voltaic battery, a combination which I made known in the year 1839; indeed, we may safely say, that when the chemical force is utilised, or not wasted, but all converted into electrical force, the more powerful the chemical action, the more powerful is the electrical action which results.

If, instead of employing manufactured products or educts, such as zinc and acids, we could realise as electricity the whole of the chemical force which is active in the combustion of cheap and abundant raw materials, such as coal, wood, fat, &c., with air or water, we should obtain one of the greatest practical desiderata, and have at our command a mechanical power in every respect superior in its applicability to the steam-engine.

I have shown that the flame of the common blowpipe gives rise to a very marked electrical current, capable not only of affecting the galvanometer, but of producing chemical decomposition: two plates or coils of platinum are placed, the one in the portion of the flame near the orifice of the jet, or at the points where combustion commences, the other in the full yellow flame where combustion is at its maximum; this latter should be kept cool, to enable a thermo-electric current, which is produced by the different temperature of the platinum plates, to co-operate with the flame current; wires attached to the plates of platinum form the terminals or poles. By a row of jets a flame battery may be formed, yielding increased effects; but in these experiments, though theoretically interesting, so small a fraction of the power, actually at work in the combustion, has been thrown into an electrical form, that there is no immediate promise of a practical result.

The quantity of the electrical current, as measured by the quantity of matter it acts upon in its different phenomenal effects, is proportionate to the quantity of chemical action which generated it; and its intensity, or power of overcoming resistance, is also proportionate to the intensity of chemical affinity when a single voltaic pair is employed, or to the number of reduplications when the well-known instrument called the voltaic battery is used.

The mode in which the voltaic current is increased in

intensity by these reduplications, is in itself a striking instance of the mutual relations and dynamic analogies of different forces. Let a plate of zinc or other metal possessing a strong affinity for oxygen, and another of platinum or other metal possessing little or no affinity for oxygen, be partially immersed in a vessel, A, containing dilute nitric acid, but not in contact with each other; let platinum wires touching each of these plates have their extremities immersed in another vessel, B, containing also dilute nitric acid: as the acid in vessel A is decomposed, by the chemical affinity of the zinc for the oxygen of the acid, the acid in vessel B is also decomposed, oxygen appearing at the extremity of the wire which is connected with the platinum: the chemical power is conveyed or transferred through the wires, and, abstracting certain local effects, for every unit of oxygen which combines with the zinc in the one vessel, a unit of oxygen is evolved from the platinum wire in the other. The platinum wire is thus thrown into a condition analogous to zinc, or has a power given to it of determining the oxygen of the liquid to its surface, though it cannot, as is the case with zinc, combine with it under similar circumstances. If we now substitute for the platinum wire, which was connected with the platinum plate, a zinc wire, we have, in addition to the determining tendency by which the platinum was affected, the chemical affinity of the oxygen in vessel B for the zinc wire: thus we have, added to the force which was originally produced by the zinc of the combination in vessel A, a second force produced by the zinc in vessel B, co-operating with the first; two pairs of zinc and platinum thus connected produce, therefore, a more intense effect than one pair; and if we go on adding to these alternations of zinc, platinum, and liquid, we obtain an indefinite exaltation of chemical power, just as in mechanics we obtain accelerated motion by adding fresh impulses to motion already generated.

The same rule of proportion which holds good in chemical combinations also obtains in electrical effects, when these are produced by chemical actions. Dalton and others proved that the constituents of a vast number of compound substances always bore a definite quantitative relation to each

other: thus, water, which consists of one part by weight of hydrogen united to eight parts of oxygen, cannot be formed by the same elements in any other than these proportions; you can neither add to nor subtract from the normal ratio of the elements, without entirely altering the nature of the compound. Further, if any element be selected as unity, the combining ratios of other elements will bear an invariable quantitative relation to that and to each other: thus, if hydrogen be chosen as 1, oxygen will be 8, chlorine will be 36; that is, oxygen will unite with hydrogen in the proportion of 8 parts by weight to 1, while chlorine will unite with hydrogen in the proportion of 36 to 1, or with oxygen in the proportion of 36 to 8. Numbers expressing their combining weights, which are thus relative, not absolute, may, by a conventional assent as to the point of unity, be fixed for all chemical reagents; and, when so fixed, it will be found that bodies, at least in inorganic compounds, generally unite in those proportions, or in simple multiples of them: these proportions are termed *Equivalents*.

Now, a voltaic battery, which consists usually of alternations of two metals, and a liquid capable of acting chemically upon one of them, has, as we have seen, the power of producing chemical action in a liquid connected with it by metals upon which this liquid is incapable of acting: in such case the constituents of the liquid will be eliminated at the surfaces of the immersed metals, and at a distance one from the other. For example, if the two platinum terminals of a voltaic battery be immersed in water, oxygen will be evolved at one and hydrogen at the other terminal, exactly in the proportions in which they form water; while, to the most minute examination, no action is perceptible in the intervening stratum of liquid. It was known before Faraday's time that, while this chemical action was going on in the subjected liquid, a chemical action was going on in the cells of the voltaic battery; but it was scarcely if at all known that the amount of chemical action in the one bore a constant relation to the amount of action in the other. Faraday proved that it bore a direct equivalent relation: that is, supposing the battery to be formed of zinc, platinum, and water, the amount

of oxygen which united with the zinc in each cell of the battery was exactly equal to the amount evolved at the one platinum terminal, while the hydrogen evolved from each platinum plate of the battery was equal to the hydrogen evolved from the other platinum terminal.

Supposing the battery to be charged with hydrochloric acid, instead of water, while the terminals are separated by water, then for every 36 parts by weight of chlorine which united with each plate of zinc, eight parts of oxygen would be evolved from one of the platinum terminals: that is, the weights would be precisely in the same relation which Dalton proved to exist in their chemical combining weights. This relation applies to all liquids capable of being decomposed by the voltaic electrical force, thence called *Electrolytes*; and as no voltaic effect is produced by liquids incapable of being thus decomposed, it follows that voltaic action is chemical action taking place at a distance, or transferred through a chain of media, and that the chemical equivalent numbers are the exponents of the amount of voltaic action for corresponding chemical substances.

As heat, light, magnetism, or motion, can be produced by the requisite application of the electric current, and as this is definitely produced by chemical action, we get these forces very definitely, though not immediately, produced by chemical action. Let us, however, here enquire, as we have already done with respect to the other forces, how far other forces may directly emanate from chemical affinity.

Heat is an immediate product of chemical affinity. I know of no exception to the general proposition that all bodies in chemically combining produce heat; *i.e.* if solution be not considered as chemical action, and even in that case, when cold results, it is from a change of consistence, as from the solid to the liquid state, and not from chemical action.

We shall find that the same view of the expenditure of force which we have considered in treating of latent heat holds good as to the expenditure of chemical force when regarded with reference to the amount of heat or repulsive force which it engenders, the chemical force being here exhausted by mechanical expansion—that is, by heat. Thus, in the

chemical action of the ordinary combustion of coal and oxygen, the expenditure of fuel will be in proportion to the expansibility of the substances heated; water passing freely into the state of steam will consume more fuel than if it be confined and kept at a temperature above its boiling-point.

Why chemical action produces heat, or what is the action of the molecules of matter when chemically uniting, is a question upon which many theories have been proposed, and which may possibly be never more than approximately resolved.

Some authors explain it by the condensation which takes place; but this will not account for the many instances where, from the liberation of gases, a great increase of volume ensues upon chemical combustion, as in the familiar instance of the explosion of gunpowder; others explain it as resulting from the union of atmospheres of positive and negative electricity, which are assumed to surround the atoms of bodies; but this involves hypothesis upon hypothesis. Dr. Wood has lately thrown out a view of the heat of chemical action which is more in accordance with a dynamic theory of heat, and as such demands some notice. Starting with his proposition, which I have previously mentioned, 'that the nearer the particles of bodies are to each other the less they require to move to produce a given motion in the particles of another body,' his argument assumes something of this form.

In the mechanical approximation of the particles of a homogeneous body heat results; the particles *a a* of the body A would, by their approximation, produce expansion in the neighbouring body B, the more so in proportion as they themselves were previously nearer to each other. In chemically combining, *a a* the particles of A are brought into very close proximity with *b b* the particles of B; heat should therefore result, and the greater because the proximity may fairly be assumed to be greater in the case of chemical combination than in that of mechanical compression. In cases, then, where there is no absolute diminution of bulk ensuing on chemical combination, if the greater proximity of the combining particles be such that the correlative expansion ought to be greater

(if there were no chemical combination) than that occupied by the total volume of the new compound, an extra expanding power is evolved, and heat or expansion ought to be produced in surrounding bodies. In other words, if *a a* could be brought by physical attraction as near each other as they are by chemical attraction brought near to *b b*, they would, from their increased proximity, produce an expansive power *ultra* the volume occupied by the actual chemical compound A and B. The question, however, immediately occurs, why should the volume of the compound be limited and not occupy the full space equivalent to the expanding power induced by the contraction or approximation of the particles? As the distance of the particles is the resultant of the contending contracting and expanding powers, this result ought to express itself in terms of the actual volume produced by the combination, which it certainly does not.

Though I see some difficulties in Dr. Wood's theory, and perhaps have not rightly conceived it, his views have to my mind great interest, his mode of regarding natural phenomena being analogous to that which I have in this Essay, and for many years, advocated, viz. to divest physical science as much as possible of hypothetic fluids, ethers, latent entities, occult qualities, &c.

Many compounds evolve heat by decomposition alone, *e.g.* chloride and iodide of nitrogen, euchlorine, &c.; this cannot well be from a falling together of the atoms. My own notion of the heat produced by chemical combination —though I scarcely dare venture an opinion upon a subject so controverted—is, that it is analogous to the heat of friction, that the particles of matter in close approximation and rapid motion *inter se* evolve heat as a continuation of the motion interrupted by the friction or intestinal motion of the particles: heat would thus be produced, whether the resulting compound were of greater or less bulk than the sum of the components, though of course when the compound is of greater bulk less heat would be apparent in neighbouring bodies, the expansion taking place in one of the substances themselves—I say in one of them, for it is stated in books of authority that there is no instance of two or more solids or

liquids, or a solid and a liquid, combining and producing a compound which is entirely gaseous at ordinary temperatures and pressures. The substance gun-cotton, however, discovered by Schoenbein, very nearly realises this proposition.

Dr. Andrews has arrived at the conclusion, after careful experiment, that in chemical combinations where acids and alkalies or analogous substances are employed, the amount of heat produced is determined by the basic ingredient, and his experiments have received general assent; although it should be stated that M. Hess arrived at contrary results, the acid constituent according to his experiments furnishing the measure of the heat developed.

Light is directly produced by chemical action, as in the flash of gunpowder, the burning of phosphorus in oxygen gas, and all rapid combustions: indeed, wherever intense heat is developed, light accompanies it. In many cases of slow combustion, such as the phenomena of phosphorescence, the light is apparently much more intense than the heat, the former being obvious, the latter so difficult of detection that for a long time it was a question whether any heat was eliminated; and I am not aware that, at the present day, any thermic effects from certain modes of phosphorescence, such as those of phosphorescent wood, putrescent fish, &c., have been detected.

Chemical action produces *magnetism* whenever it is thrown into a definite direction, as in the phenomenon of electrolysis. I may adduce the gas voltaic battery, as presenting a simple instance of the direct production of magnetism by chemical synthesis. Oxygen and hydrogen in that combination chemically unite; but instead of combining by intimate molecular admixture, as in the ordinary cases, they act upon water, *i.e.* combined oxygen and hydrogen, placed between them so as to produce a line of chemical action; and a magnet adjacent to this line of action is deflected, and places itself at right angles to it. What a chain of molecules does here, there can be no doubt, all the molecules entering into combination would produce in ordinary chemical actions; but in such cases, the direction of the lines of combination being irregular and confused, there is no general resultant by which the magnet can be affected.

What the exact nature of the transferrence of chemical power across an electrolyte is, we at present know not, nor can we form any more definite idea of it than that given by the theory of Grotthus. We have no knowledge as to the exact nature of any mode of chemical action, and, for the present, must leave it as an obscure action of force, of which future researches may simplify our apprehensions.

We have seen that an equivalent or proportionate electrical effect is produced by a given amount of chemical action; if we, in turn, produce heat, magnetism, and motion by the electricity resulting from chemical action, we shall be able to measure these forces far more accurately than when they are directly produced, and thus to deduce their equivalent relation to the initial chemical action. Thus, M. Favre, after ascertaining the quantity of heat produced by the oxidation of a given quantity of zinc, and finding, as have others, that the heat so produced is the same in amount as when evolved from a voltaic battery by the same consumption of zinc forming its positive element, makes the following experiment.

A voltaic battery and electro-magnet are immersed in calorimeters, and the heat produced when the connection with the magnet is effected is noted.

The electro-magnet is then made to raise a weight, and thus perform mechanical work; and the heat produced is again noted. It is found in the latter case that less heat is evolved than in the former; a certain quantity of heat has therefore been replaced by the mechanical work; and by estimating the amount of heat subtracted, and the amount of work produced, he deduces the relative equivalent of work to heat. These experiments give a production of mechanical work by chemical action, not, it is true, a direct production, but, as the heat and work are in inverse ratios, and each has its source in chemical action, they prove that they are definite for a definite amount of chemical action; and as each is produced respectively by electricity and magnetism, these forces must also bear a definite relation to the initial chemical force.

The doctrine of definite combining proportions, which so beautifully serves to relate chemistry to voltaic electricity, led to the atomic theory, which, though adopted in its universality

by a large majority of chemists, presents great difficulties when extended to all chemical combinations.

The equivalent ratios in which a great number of substances chemically combine, hold good in so many instances, that the atomic doctrine is believed by many to be universally applicable, and called a law; and yet, when followed in the combinations of substances whose mutual chemical attractions are very feeble, the relation fades away, and is sought to be recovered by applying a separate and arbitrary multiplier to the different constituents.

Thus, when it was found that a vast number of substances combined in definite volumes and weights, and in definite volumes and weights only, it was argued that their ultimate molecules or atoms had a definite size and weight, as otherwise there was no apparent reason why this equivalent ratio should hold good? why, for instance, water should only be formed of two volumes or one unit by weight of hydrogen, and of one volume or eight units by weight of oxygen? why, unless there were some ultimate limits to the divisibility of its molecules, should not water, or a fluid substance approximating to water in character, be formed by a half, a third, or a tenth part of hydrogen, with eight parts of oxygen?

It was perfectly consistent with the atomic view that a substance might be formed with one part combined with eight parts, or with sixteen, or with twenty-four, for in such a substance there would be no subdivision of the (supposed indivisible) molecule; and this held good with many compounds: thus, fourteen parts by weight, say grains of nitrogen, will combine respectively with eight, sixteen, twenty-four, thirty-two, and forty parts by weight, or grains, of oxygen.

So, again, twenty-seven grains of iron will combine with eight grains of oxygen or with twenty-four grains, *i.e.* three proportionals of oxygen. No compound is known in which twenty-seven grains of iron will combine with two proportionals or sixteen grains of oxygen; but this does not much affect the theory, as such a compound may be yet discovered, or there may be reasons at present unknown why it cannot be formed.

But now comes a difficulty: twenty-seven parts by weight

of iron *will* combine with twelve parts by weight of oxygen, and twenty-seven parts of iron will also combine with ten and two-third parts of oxygen. Thus, if we retain the unit of iron we must subdivide the unit of oxygen, or if we retain the unit of oxygen we must subdivide the unit of iron, or we must subdivide both by a different divisor. What then becomes of the notion of an atom or molecule physically indivisible?

If iron were the only substance to which this difficulty applied, it might be viewed as an unexplained exception, or as a mixture of two oxides; or recourse might be had to a more minute subdivision to form the units or equivalents of other substances; but numerous other substances fall under a similar category; and in organic combinations, to preserve the atomic nomenclature, we must apply a separate multiplier or divisor to far the greater number of the elementary constituents, *i.e.* we must divide that which is, *ex hypothesi*, indivisible.

Thus, to take a more complex substance than any formed by the combination of iron and oxygen, let us select the substance albumen, composed of carbon, hydrogen, nitrogen, oxygen, phosphorus, and sulphur. In this case we must either divide the atoms of phosphorus and sulphur so as to reduce them to small fractions, or multiply the atoms of the other substances by extravagant numbers; thus, to preserve the unit of one of the constituents of this substance, chemists say it is composed of 400 atoms of carbon, 310 of hydrogen, 120 of oxygen, 50 of nitrogen, 2 of sulphur, and 1 of phosphorus. This is a somewhat extreme case, but similar difficulties will be found in different degrees to prevail among organic compounds; in very many no constituent can be taken as a unit to which simple multiples of any of the others will give their relative proportions. By the mode of notation adopted, if any conceivable substance be selected, it could, whatever be the proportions of its constituents, be formulated as atomic. A solution of an ounce of sugar in a pound of water, in a pound and a half, in a pound and a quarter, in a pound and a tenth, might be expressed in an atomic form, if we select arbitrarily a multiplier or divisor.

In the case of solution, different proportions can be united up to the point of saturation without any difference

in the character of the compound, and the same may be predicated to some extent of an acid and an alkali; but even where the steps are sudden, and compounds only exist with definite proportions, they cannot, in a multitude of cases, be reconciled with the true idea of an atomic combination, *i.e.* one to one, one to two, one to three, &c.

Although, therefore, there are facts which show that there is some restrictive law of combination which in numerous cases limits the ratios in which substances will combine, nay, further, which show many instances of a proportion between the combining weights of one compound and those of another; although there is also a remarkable simplicity in the combining volumes of numerous gases, there remain very many cases to which the doctrine of atomic combinations cannot fairly be applied.

That there must be something in the constitution of matter, or in the forces which act on it, to account for the *per saltum* manner in which chemical combinations take place, is inevitable; but the idea of atoms does not seem satisfactorily to account for it.

By selecting a separate multiplier or divisor, chemists may denote every combination in terms derived from the atomic theory; but they have passed from the original law, which contemplated only definite multiples, and the very hypothetic expressions of atoms, which the apparently simple relations of combining weights first led them to adopt, they are obliged to vary and to contradict in terms, by dividing that which their hypothesis and the expression of it assumed to be indivisible.

While, therefore, I fully recognise a great natural truth in the definite ratios presented by a vast number of chemical combinations, and in the *per saltum* steps in which nearly all take place, I cannot join in evading the argument against the atomic theory presented by those combinations which are made to support it only by the application of an arbitrary notation.

A similar straining of theory seems gradually obtaining in regard to the doctrine of compound radicals. The discovery of cyanogen by Gay-Lussac was probably the first inducement to the doctrine of compound radicals: a doctrine which is now generally, perhaps too generally, received in

organic chemistry. As, in the case of cyanogen, a body obviously compound discharged in almost all its reactions the functions of an element, so in many other cases it was found that compound bodies, in which a number of elements existed, might be regarded as binary combinations, by considering certain groups of these elements as a compound radical; that is, as a simple body when treated of in relation to the other complex substances of which it forms part, and only as non-elementary when referred to its own internal constitution.

Undoubtedly, by approximating in theory the reactions of inorganic and of organic chemistry, by keeping the mind within the limits of a beaten path, instead of allowing it to wander through a maze of isolated facts, the doctrine of compound radicals has been of service; but, on the other hand, the indefinite variety of changes which may be rung upon the composition of an organic substance, by different associations of its primary elements, makes the binary constituents vary as the minds of the authors who treat of them, and makes their grouping depend entirely upon the strength of the analogies presented to each individual mind. From this cause, and from the extreme license which has been taken in theoretic groupings deduced from this doctrine, a serious question arises whether it may not ultimately, unless carefully restricted, produce confusion rather than simplicity, and be to the student an embarrassment rather than an assistance.

The term 'law' has not unfrequently been applied to groupings of phenomena such as those to which I have been referring; it seems to me that a popular misconception as to the meaning of the term prevails. Putting aside what are called necessary truths, such as those proved by mathematics, and the vexed metaphysical question as to whether these are or are not independent of experience, we can, I think, have little doubt that a vast number of those formulæ to which the term 'laws' are commonly applied are merely categories into which our own experience has thrown phenomenal instances. The so-called law of chemical equivalents, the laws of refraction and polarisation of light, of the reciprocal action and reaction of electricity and magnetism, nay, the very grouping of phenomena under the terms electricity, magnetism, &c.,

are mere results of collected and collated observations, and all our generalising philosophy is based on an unconscious belief in continuity. In such cases, though the word 'law' is used, all that can be predicated is that, as far as experience goes, certain results follow certain arrangements of matter. Many of these supposed laws have been found to be erroneous or to be no laws at all, *e.g.* Marriotte's law as to the expansion of gases, Gay-Lussac's as to the chemical relations of the volumes of gases, &c.; the word 'law,' therefore, excepting the term be applied to mathematical truths, is merely a generalised expression of certain observed results which as far as our experience goes are invariable ; and when we personify the term 'law,' as by talking of effects taking place in obedience to a law, we use an inaccurate expression, only justifiable as metaphor ; but this use of the term has so warped men's minds that the mass of even educated people believe in law as something separate from and governing the phenomena, the latter being supposed to be subject to the law, whereas this is only an abstract definition of relations derived from the observation of the phenomena themselves. The word 'nature' is still more personified ; instead of being used to denote, what alone it can denote—namely, things as we see, hear or feel, them, and their relations ascertained by comparison and abstraction—nature is treated as a sort of superintending angel who enjoins this, permits that, and forbids the other.

It may be that natural phenomena, as we come to know them more thoroughly, may resolve themselves into forms which may be mathematically expressed, and which may have the certainty of mathematical truths, as the laws of Kepler and Newton's law of gravitation now appear to have; but we are far from that at present so far as regards the greater number of phenomena, particularly those exhibited by molecular and physiological effects. To bring all within the domain of what are termed necessary truths would require omniscience.

OTHER MODES OF FORCE.

Catalysis, or the chemical action induced by the mere presence of a foreign body, embraces a class of facts which must considerably modify many of our notions of chemical action: thus, oxygen and hydrogen, when mixed in a gaseous state, will remain unaltered for an indefinite period; but the introduction to them of a slip of clean platinum will cause more or less rapid combination, without being in itself in any respect altered. On the other hand, oxygenated water, which is a compound of one equivalent of hydrogen plus two of oxygen, will, when under a certain temperature, remain perfectly stable; but touch it with platinum in a state of minute division, and it is instantly decomposed, one equivalent of oxygen being set free. Here, again, the platinum is unaltered, and thus we have both synthesis and analysis effected apparently by the mere contact of a foreign body. It is not improbable that the increased electrolytic power of water by the addition of some acids, such as the sulphuric and phosphoric, where the acids themselves are not decomposed, depends upon a catalytic effect of these acids; but we know too little of the nature and rationale of catalysis to express any confident opinion on its modes of action, and possibly we may comprehend very different molecular actions under one and the same name. In no case does catalysis yield us new power or force: it only determines or facilitates the action of chemical force, and, therefore, is no creation of force by contact.

The force so developed by catalysis may be converted into a voltaic form thus: in a single pair of the gas battery previously alluded to, one portion of a strip of platinum is immersed in a tube of oxygen, the other in one of hydrogen, both the gases and the extremities of the platinum being connected by water or other electrolyte; a voltaic combina-

tion is thus formed, and electricity, heat, light, magnetism, and motion produced at the will of the experimenter.

In this combination we have a striking instance of correlative expansions and contractions, analogous, though in a much more refined form, to the expansions and contractions by heat and cold detailed in the early part of this Essay, and illustrated by the alternate actions of two bladders partially filled with air: thus, as by the effect of chemical combination in each pair of tubes of the gas battery the gases oxygen and hydrogen lose their gaseous character and shrink into water, so at the platinum terminals of the battery, when immersed in water, water is decomposed, and expands into oxygen and hydrogen gases. The correlate of the force which changes gas into liquid at one point of space changes liquid into gas at another, and the exact volume which disappears in the one place reappears in the other; so that it would appear to an inexperienced eye as though the gases passed through solid wires.

Gravitation, inertia, and aggregation were but cursorily alluded to in my original Lectures; their relation to the other modes of force seemed to be less definitely traceable; but the phenomenal effects of gravitation and inertia, being motion and resistance to motion, in considering motion I have in some degree included their relations to the other forces.

To my mind gravitation would only produce other force when the motion caused by it is diminished or ceases. Thus, if we suppose a meteor to be a mass rotating in an orbit round the earth, and with no resisting medium, then, as long as that rotation continues, the motion of the meteoric mass itself would be the exponent of the force impelling it; if there be a resisting medium, part of this motion would be arrested and taken up by the medium, either as motion, heat, electricity, or some other mode of force; if the meteor approach the earth sufficiently to fall upon it, the perceptible motion of the meteor is stopped, but is taken up by the earth which vibrates through its mass; part also reappears as heat in both earth and meteor, and part in the change in the earth's position consequent on its increase of gravity, and so on. Gravitation is but the subjective idea, and its relation to other modes of

force seems to me to be identical with that of pressure or motion. Thus, when arrested motion produces heat, it matters not whether the motion has been produced by a falling body, *i.e.* by gravitation, or a body projected by an explosive compound, &c.; the heat will be the same, provided the mass and velocity at the time of arrest be the same. In no other sense can I conceive a relation between gravitation and the other forces, and, with all diffidence, I cannot agree with those who consider there is a different sort of link.

Mosotti has mathematically treated of the identity of gravitation with *cohesive attraction*, and Plücker has succeeded in showing that crystalline bodies are definitely affected by magnetism, and take a position in relation to the lines of magnetic force dependent upon their optical axis or axis of symmetry.

What is termed the optic axis is a fixed direction through crystals in which they do not doubly refract light, and which direction, in those crystals which have one axis of figure, or a line around which the figure is symmetrical, is parallel to the axis of symmetry. When submitted to magnetic influence such crystals take up a position, so that their optic axis points diamagnetically or transversely to the lines of magnetic force; and when, as is the case in some crystals, there is more than one optic axis, the resultant of these axes points diamagnetically. The mineral cyanite is influenced by magnetism in so marked a manner that when suspended it will arrange itself definitely with reference to the direction of terrestrial magnetism, and may, according to Plücker, be used as a compass-needle.

There is scarcely any doubt that the force which is concerned in *aggregation* is the same which gives to matter its crystalline form; indeed, a vast number of inorganic bodies, if not all, which appear amorphous, are, when closely examined, found to be crystalline in their structure: we thus get a reciprocity of action between the force which unites the molecules of matter and the magnetic force, and through the medium of the latter the correlation of the attraction of aggregation with the other modes of force may be established.

I believe that the same principles and mode of reasoning

as have been adopted in this Essay might be applied to the organic as well as the inorganic world; and that muscular force, animal and vegetable heat, &c. might, and at some time will, be shown to have similar definite correlations; but I have purposely avoided this subject, as pertaining to a department of science to which I have not devoted my attention. I ought, however, while alluding to this subject, shortly to mention some experiments of Professor Matteucci, communicated to the Royal Society in the year 1850, by which it appears that whatever mode of force it be which is propagated along the nervous filaments, this mode of force is definitely affected by currents of electricity. His experiments show that when a current of positive electricity traverses a portion of the muscle of a living animal in the same direction as that in which the nerves ramify—*i.e.* a direction from the brain to the extremities—a muscular contraction is produced in the limb experimented on, showing that the nerve of motion is affected; while, if the current, as it is termed, be made to traverse the muscle in the reverse direction, or towards the nervous centres, the animal utters cries, and exhibits all the indications of suffering pain, scarcely any muscular movement being produced, showing that in this case the nerves of sensation are affected by the electric current; some definite polar condition therefore exists, or is induced in the nerves, to which electricity is co-related, and probably this polar condition constitutes or conveys nervous agency. There are other analogies given in the papers of M. Matteucci, and derived from the action of the electrical organs of fishes, which tend to corroborate and develop the same view.

By an application of the doctrine of the Correlation of Forces, Dr. Carpenter has shown how a difficulty arising from the ordinary notions of the development of an organised being from its germ-cell may be lessened. It has been thought by many physiologists that the *nisus formativus*, or organising force of an animal or vegetable structure, lies dormant in the primordial germ-cell. 'So that the organising force required to build up an oak or a palm, an elephant or a whale, is concentrated in a minute particle only discernible by microscopic aid.'

Certain other views of nearly equal difficulty have been propounded. Dr. Carpenter suggests the probability of extraneous forces, as heat, light, and chemical affinity, continuously operating upon the material germ; so that all that is required in this is a structure capable of receiving, directing, and converting these forces into those which tend to the assimilation of extraneous matter and the definite development of the particular structure. In proof of this position he shows how dependent the process of germ development is upon the presence and agency of external forces, particularly heat and light, and how it is regulated by the measure of these forces supplied to it.

It certainly is far less difficult so to conceive the supply of force yielded to organised beings in their gradual process of growth, than to suppose a store of dormant or latent force pent up in a microscopic monad.

As by the artificial structure of a voltaic battery, chemical actions may be made to co-operate in a definite direction, so, by the organism of a vegetable or animal, the mode of motion which constitutes heat, light, &c. may, without extravagance, be conceived to be appropriated and changed into the forces which induce the absorption and assimilation of nutriment, and into nervous agency and muscular power. Indications of similar thoughts may be detected in the writings of Liebig.

Some difficulty in studying the correlations of vital with inorganic physical forces arises from the effects of sensation and consciousness, presenting a similar confusion to that alluded to, when, in treating of heat, I ventured to suggest that observers are too apt to confound the sensations with the phenomena. Let us apply some of the considerations on force, given in the introductory portion of this Essay, to cases where vitality or consciousness intervenes. When a weight is raised by the hand, there should, according to the doctrine of non-creation of force, have been somewhere an expenditure equivalent to the amount of gravitation overcome in raising the weight. That there is expenditure we can prove, though in the present state of science we cannot measure it. Thus, prolong the effort, raise weights for an hour or two, the vital

powers sink, food, *i.e.* fresh chemical force, is required to supply the exhaustion. If this supply is withheld and the exertion is continued, we see the consumption of force in the supervening weakness and emaciation of the body.

The consciousness of effort, which has formed a topic of argument by some writers when treating of force, and is by them believed to be that which has originated the idea of force, may by the physical student be regarded as feeling is in the phenomena of heat and cold, viz. a sensation of the struggle of opposing molecular motions in overcoming the resistance of the masses to be moved. When we say we feel hot, we feel cold, we feel that we are exerting ourselves, our expressions are intelligible to beings who are capable of experiencing similar sensations ; but the physical changes accompanying these sensations are not thereby explained. Without pretending to know what probably we shall never fully know, the actual *modus agendi* of the brain, nerves, muscles, &c., we may study vital as we do inorganic phenomena, both by observation and experiment. Thus Sir Benjamin Brodie has examined the effect of respiration on animal heat by inducing artificial respiration after the spinal cord has been severed ; in which case he finds the animal heat declines, notwithstanding the continuance of the chemical action of respiration, carbonic acid being formed as usual ; but he also finds that under such circumstances the struggles or muscular actions of the animal are very great, and sufficient probably to account for the force eliminated by the chemical action in digestion and respiration ; and Liebig, by measuring the amount of chemical action in digestion and respiration, and comparing it with the labour performed, has to some extent established their equivalent relations.

Mr. Helmholtz has found that the chemical changes which take place in muscles are greater when these are made to undergo contractions than when they are in repose ; and that, as would be expected, the consumption of the matter of the muscle, or, in other terms, the waste of excrementitious matter thrown off, is greater in the former than in the latter case.

M. Matteucci has ascertained that the muscles of recently

killed frogs absorb oxygen and exhale carbonic acid, and that when they are thrown into a state of contraction, and still more when they perform mechanical work, the absorption is increased ; and he even calculates the equivalents of work so performed.

M. Beclard finds that the quantity of heat produced by voluntary muscular contraction in man is greater when that contraction is what he terms static—that is, when it produces no external work, but is effort alone—than when that effort and contraction are employed dynamically, so as to raise a weight or produce mechanical work. Every year is now adding evidence that the forces brought into play by organised beings are traceable to the continuous and mainly to the chemical effects of food, air, light, &c. The assimilation of carbon by plants, that of carbon and nitrogen by animals, together with the slow combustion by inhalation of oxygen ; and the derivation of the forces which animate and keep alive organisms, from the heat and light radiated by the sun, are now becoming accepted beliefs.

Thus, though we may see no present promise of being able to resolve sensations into their ultimate elements, or to trace, physically, the link which unites volition with exertion or effort, we may hope to approximate the solution of these deeply interesting questions.

In the same individual the chemical and physical state of the secretions in the warm may be compared with those in the cold parts of the body. The changes in digestion and respiration, when the body is in a state of rest, may be compared with those which obtain when it is in a state of activity. The relations with external matter, maintaining, by the constant play of natural forces, the vital nucleus, or the organisation by means of which matter and force receive, for a definite period, a definite incorporation and direction, may be ascertained, while the more minute structural changes are revealed to us by the ever-improving powers of the microscope ; and thus step by step we may learn that which it is given to us to learn, boundless in its range and infinite in its progress, and therefore never giving a response to the ultimate—Why ?

As the first glimpse of a new star is caught by the eye of

the astronomer while directing his vision to a different point of space, and disappears when steadfastly gazed at, only to have its position and figure ultimately ascertained by the employment of more penetrative powers, so the first scintillations of new natural phenomena frequently present themselves to the eye of the observer, dimly seen when viewed askance, and disappearing if directly looked for. When new powers of thought and experiment have developed and corrected the first notions, and given a character to the new image probably very different from the first impression, fresh objects are again glanced at in the margin of the new field of vision, which in their turn have to be verified, and again lead to new extensions; thus, the effort to establish one observation leads to the imperfect perception of new and wider fields of research; and, instead of approaching finality, the more we discover the more infinite appears the range of the undiscovered!

CONCLUDING REMARKS.

I HAVE now gone through the affections of matter to which distinct names have been given in our received nomenclature: that other forces may be detected, differing as much from them as they differ from each other, is highly probable, and that when discovered, and their modes of action fully traced out, they will be found to be related *inter se*, and to these forces as these are to each other, I believe to be as far certain as certainty can be predicted of any future event.

It may in many cases be a difficult question to determine what constitutes a distinct affection of matter or mode of force. It is highly probable that different lines of demarcation would have been drawn between the forces already known, had they been discovered in a different manner, or first observed at different points of the chain which connects them. Thus, radiant heat and light are mainly distinguished by the manner in which they affect our senses: were they viewed according to the way in which they affect inorganic matter, very different notions would possibly be entertained of their character and relation. Electricity, again, was named from the substance in which, and magnetism from the district where, they first happened to be observed, and a chain of intermediate phenomena has so connected electricity with galvanism that they are now regarded as the same force, differing only in the degree of its intensity and quantity, though for a long time they were regarded as distinct.

The phenomenon of attraction and repulsion by amber, which originated the term *electricity*, is as unlike that of the decomposition of water by the voltaic pile, as any two natural phenomena can well be. It is only because the historical sequence of scientific discoveries has associated them by a number of intermediate links, that they are classed within the

same category. What is called voltaic electricity might equally, perhaps more appropriately, be called voltaic chemistry. I mention these facts to show that the distinction in the name may frequently be much greater than the distinction of the subject which it represents, and *vice versâ*, not as at all objecting to the received nomenclature on these points; nor do I say it would be advisable to depart from it: were we to do so, inevitable confusion would result, and objections equally forcible might be found to apply to our new terminology.

Words, when established to a certain point, become a part of the social mind; its powers and very existence depend upon the adoption of conventional symbols; and were these suddenly departed from, or varied, according to individual apprehensions, the acquisition and transmission of knowledge would cease. Undoubtedly, neology is more permissible in physical science than in any other branch of knowledge, because it is more progressive; new facts or new relations require new names, but even here it should be used with great caution.

> Si forte necesse est
> Indiciis monstrare recentibus abdita rerum,
> Fingere cinctutis non exaudita Cethegis
> Continget; dabiturque licentia, sumpta pudenter.

Even should the mind ever be led to dismiss the idea of various forces, and regard them as the exertion of one force, or resolve them definitely into motion, still we could never avoid the use of different conventional terms for the different modes of action of this one pervading force.

Reviewing the series of relations between the various forces which we have been considering, it would appear that in many cases where one of these is excited or exists, all the others are also set in action: thus, when a substance, such as sulphuret of antimony, is electrified, at the instant of electrisation it becomes *magnetic* in directions at right angles to the lines of electric force; at the same time it becomes *heated* to an extent greater or less according to the intensity of the electric force. If this intensity be exalted to a certain point the sulphuret becomes luminous, or *light* is produced: it expands, consequently *motion* is produced; and it is decomposed, therefore

chemical action is produced. If we take another substance, say a metal, all these forces except the last are developed; and although we can scarcely apply the term chemical action to a substance hitherto undecomposed, and which, under the circumstances we are considering, enters into no new combination, yet a metal undergoes that species of polarisation which, as far as we can judge, is the first step towards chemical action, and which, if the substance were decomposable, would resolve it into its elements. Perhaps, indeed, some hitherto undiscovered chemical action is produced in substances which we regard as undecomposable: there are experiments, to some of which I have alluded, which tend to show that metals which have been electrised are permanently changed in their molecular constitution. Oxygen, we have seen, is changed by the electric spark into ozone, and phosphorus into allotropic phosphorus, both which changes were for a long time unknown to those familiar with electrical science.

Thus, with some substances, when one mode of force is produced all the others are simultaneously developed. With other substances, probably with all matter, some of the other forces are developed, whenever one is excited, and all may be so were the matter in a suitable condition for their development, or our means of detecting them sufficiently delicate.

This simultaneous production of several different forces seems at first sight to be irreconcilable with their mutual and necessary dependence, and it certainly presents a formidable experimental difficulty in the way of establishing their equivalent relations; but when examined closely, it is not in fact inconsistent with the views we have been considering, but is indeed a strong argument in favour of the theory which regards them as modes of motion.

Let us select one or two cases in which this form of objection may be prominently put forward. A voltaic battery decomposing water in a voltameter, while the same current is employed at the same time to make an electro-magnet, gives nevertheless in the voltameter an equivalent of gas, or decomposes an equivalent of an electrolyte for each equivalent of chemical decomposition in the battery cells, and will give the same ratios if the electro-magnet be removed. Here, at

first sight, it would appear that the magnetism was an extra force produced, and that thus more than the equivalent power was obtained from the battery. In answer to this objection it may be said, that in the circumstances under which this experiment is ordinarily performed, several cells of the battery are used, and so there is a far greater amount of heat, &c., generated in the cells than is indicated by the effect in the voltameter. If, moreover, the magnet be not interposed, still the magnetic force is equally existent throughout the whole current; for instance, the wires joining the plates will attract iron filings, deflect magnetic needles, &c., and produce diamagnetic effects on surrounding matter. By the iron core a small portion of the force is, indeed, absorbed *while* it is being made a magnet, and probably less heat is developed in the circuit, but this ceases to be absorbed when the magnet is made; this has been proved by the observation of Mr. Latimer Clarke, who has found that along the wires of the electric telegraph the magnetic needles placed at different stations remained fixed after the connection with the battery was made, and while the electric current acted by induction on surrounding conducting matter, separated from the wires by their gutta-percha coating, so that a sort of Leyden phial was formed; but as soon as this induction had produced its effect between each station, or, so to speak, the phial was charged, the needles successively were deflected: it is like the case of a pulley and weight, which latter exhausts force while it is being raised; but when raised, the force is free, and may be used for other purposes.

If a battery of one cell, just capable of decomposing water and no more, be employed, this will cease to decompose while making a magnet. There must, in every case, be preponderating chemical affinity in the battery cells, either by the nature of its elements or by the reduplication in series, to effect decomposition in the voltameter; and if the point is just reached at which this is effected, and the power is then reduced by any resistance, decomposition ceases: were it otherwise, were the decomposition in the voltameter the exponent of the entire force of the generating cells, and these could independently produce magnetic force, this latter force would be got from nothing, and perpetual motion be obtained.

To take another and different example: A piece of zinc dissolved in dilute sulphuric acid gives somewhat less heat than when the zinc has a wire of platinum attached to it, and is dissolved by the same quantity of acid. The argument is deducible that, as there is more electricity in the second than in the first case, there should be less heat; but as, according to our received theories, the heat is a product of the electric current, and in consequence of the impurity of zinc electricity is generated in the first case molecularly, by what is called local action, though not thrown into a general direction, there should be more of both heat and electricity in the second than in the first case, as the heat and electricity due to the voltaic combination of zinc and platinum are added to that excited on the surface of the zinc, and the zinc should be, as in fact it is, more rapidly dissolved; so that the extra heat and electricity are produced by extra chemical force. Many additional cases of a similar description might be suggested. But although it is difficult, and perhaps impossible, to restrict the action of any one force to the production of one other force, and of one only—yet if the whole of one force, say chemical action, be supposed to be employed in producing its full equivalent of another force, say heat, then as this heat is capable in its turn of reproducing chemical action, and in the limit, a quantity equal or practically all but equal to the initial force: if this could at the same time produce independently another force, say magnetism, we could, by adding the magnetism to the total heat, get more than the original chemical action, and thus create force or obtain perpetual motion.

The term Correlation, which I selected as the title of my Lectures in 1843, strictly interpreted, means a necessary mutual or reciprocal dependence of two ideas, inseparable even in mental conception: thus, the idea of height cannot exist without involving the idea of its correlate, depth; the idea of parent cannot exist without involving the idea of offspring. The word itself had not been previously used; and although, as I have said, I object to the introduction of new terms without strong reason, there are a vast variety of physical relations which cannot certainly be so well expressed by any other term. The extent to which it has been since

used has, I think, justified me. Its use has, in my judgment, been carried too far in applying it to subjects quite beyond its fair meaning. There are many facts, one of which cannot take place without involving the other; one arm of a lever cannot be depressed without the other being elevated—the finger cannot press the table without the table pressing the finger. A body cannot be heated without another being cooled, or some other force being exhausted in an equivalent ratio to the production of heat; a body cannot be positively electrified without some other body being negatively electrified, &c. To such cases the term correlation may be usefully applied, but hardly to adaptations of structure, &c.

The probability is, that, if not all, the greater number of physical phenomena are, in one sense, correlative, and that, without a duality of conception, the mind cannot form an idea of them: thus, motion cannot be perceived or probably imagined without parallax or relative change of position. The world was believed fixed, until, by comparison with the celestial bodies, it was found to change its place with regard to them: had there been no perceptible matter external to the world, we should never have discovered its motion. In sailing along a river, the stationary vessels and objects on the banks seem to move past the observer: if at last he arrives at the conviction that he is moving, and not these objects, it is by correcting his senses by reflection derived from a more extensive previous use of them: even then he can only form a notion of the motion of the vessel he is in, by its change of position with regard to the objects it passes—that is, provided his body partakes of the motion of the vessel, which it only does when its course is perfectly smooth, otherwise the relative change of position of the different parts of the body and the vessel inform him of its alternating, though not fully of its progressive movement. So in all physical phenomena, the effects produced by motion are all in proportion to the relative motion: thus, whether the rubber of an electrical machine be stationary, and the cylinder mobile, or the rubber mobile and the cylinder stationary, or both mobile in different directions, or in the same direction with different degrees of velocity, the electrical effects are, *cæteris paribus*, precisely the same, pro-

vided the relative motion is the same, and so, without exception, of all other phenomena. The question of whether there can be absolute motion, or, indeed, any absolute isolated force, is purely the metaphysical question of idealism or realism—a question for our purpose of little import; sufficient for the purely physical enquirer, the maxim, '*De non apparentibus et non existentibus eadem est ratio.*'

The sense I have attached to the word Correlation, in treating of physical phenomena, will, I think, be evident, from the previous parts of this Essay, to be that of a necessary reciprocal production; in other words, that any force capable of producing another may, in its turn, be produced by it—nay, more, can be itself resisted by the force it produces, in proportion to the energy of such production, as action is ever accompanied and resisted by reaction: thus, the action of an electro-magnetic machine is reacted upon by the magneto-electricity developed by its action.

To many, however, of the cases we have been considering, the term correlation may be applied in a more strict accordance with its original sense: thus, with regard to the forces of electricity and magnetism in a dynamic state, we cannot electrise a substance without magnetising it—we cannot magnetise it without electrising it—each molecule, the instant it is affected by one of these forces, is affected by the other; but, in transverse directions, the forces are inseparable and mutually dependent—correlative, but not identical.

The evolution of one force or mode of force into another has induced many to regard all the different natural agencies as reducible to unity, and as resulting from one force which is the efficient cause of all the others: thus, one author writes to prove that electricity is the cause of every change in matter; another, that chemical action is the cause of everything; another, that heat is the universal cause, and so on. If, as I have stated it, the true expression of the fact is, that each mode of force is capable of producing the others, and that none of them can be produced but by some other as an anterior force, then any view which regards either of them as abstractedly the efficient cause of all the rest, is erroneous: the view has, I believe, arisen from a confusion between the

abstract or generalised meaning of the term cause, and its concrete or special sense; the word itself being indiscriminately used in both these senses.

Another confusion of terms has arisen, and has, indeed, much embarrassed me in enunciating the propositions put forth in these pages, on account of the imperfection of scientific language; an imperfection in great measure unavoidable, it is true, but not the less embarrassing. Thus, the words, light, heat, electricity, and magnetism, are constantly used in two senses—viz. that of the force producing, or the subjective idea of force or power, and of the effect produced, or the objective phenomenon. The word motion, indeed, is only applied to the effect, and not to the force, and the term chemical affinity is generally applied to the force, and not to the effect; but the other four terms are, for want of a distinct terminology, applied indiscriminately to both.

I may have occasionally used the same word at one time in a subjective, at another in an objective sense; all I can say is, that this cannot be avoided without a neology, which I have not the presumption to introduce, or the authority to enforce. Again, the use of the term forces in the plural might be objected to by those who do not attach to the term force the notion of a specific agency, but of one universal power associated with matter, of which its various phenomena are but diversely modified effects.

Whether the imponderable agents, viewed as force, and not as matter, ought to be regarded as distinct forces or as distinct modes of force, is probably not very material, for, as far as I am aware, the same result would follow either view; I have therefore used the terms indiscriminately, as either happened to be the more expressive for the occasion.

Throughout this Essay I have placed motion in the same category as the other affections of matter. The course of reasoning adopted in it, however, appears to me to lead inevitably to the conclusion that these affections of matter are themselves modes of motion; that, as in the case of friction, the gross or palpable motion, which is arrested by the contact of another body, is subdivided into molecular motions or vibrations, which vibrations are heat or electricity as the case

may be; so the other affections are only matter moved or molecularly agitated in certain definite directions. I have already considered the hypothesis that the passage of electricity and magnetism causes vibrations in an ether permeating the bodies through which the current is transmitted, *i.e.* the application of the same ethereal hypothesis to these imponderables which had previously been applied to light; many, in speaking of some of their effects, admit that electricity and magnetism cause or produce by their passage vibrations in the particles of matter, but regard the vibrations produced as an occasional, though not always a necessary, effect of the passage of electricity, or of the increment or decrement of magnetism. The view which I have taken is, that such vibrations, molecular polarisations, or motions of some sort from particle to particle, are themselves electricity or magnetism; or, to express it in the converse, that dynamic electricity and magnetism are themselves motion, and that permanent magnetism, and static electricity, are conditions of force bearing a similar relation to motion which tension or gravitation do. The so-called imponderables affect matter in all its states. Gases which have transmitted *light* are altered, *e.g.* chlorine is rendered capable of combining with hydrogen, liquids are altered, peroxalate of iron is chemically changed and gives off carbonic acid, and the light which has produced these effects is less able to produce them a second time. Solids are altered, as shown in the extensive range of photographic effects. So with *electricity*, compound gases are changed chemically, *e.g.* ammonia or carbonic acid; elementary gases are changed allotropically, *e.g.* phosphorus vapour or oxygen; liquids are changed, as in the electrolysis of water, &c.; and solids are changed, as in the projection of particles of the terminals, and molecular impressions on polished surfaces. Chemical affinity is only known to us by its affecting matter in the three states, and magnetism is proved to affect liquids and solids, though as yet gases are not shown to be altered by it.

This theory might well be discussed in greater detail than has been used in this work; but to do this and to anticipate objections would lead into specialties foreign to my present object, in the course of this Essay my principal aim having

been rather to show the relation of forces as evinced by acknowledged facts, than to enter upon any detailed explanation of their specific modes of action.

Probably man will never know the ultimate structure of matter or the minutiæ of molecular actions; indeed, it is scarcely conceivable that the mind can ever attain to this knowledge; the monad irresolvable by a given microscope may be resolved by an increase in power. Much harm has already been done by attempting hypothetically to dissect matter and to discuss the shapes, sizes, and numbers of atoms, and their atmospheres of heat, ether, or electricity.

Whether the regarding electricity, light, magnetism, &c., as simply motions of ordinary matter be or be not admissible, certain it is, that all past theories have resolved, and all existing theories do resolve, the actions of these forces into motion. Whether it be that, on account of our familiarity with motion, we refer other affections to it, as to a language which is most easily construed and most capable of explaining them, or whether it be that it is in reality the only mode in which our minds, as contradistinguished from our senses, are able to conceive material agencies, certain it is that since the period at which the mystic notions of spiritual or preternatural powers were applied to account for physical phenomena, all hypotheses framed to explain them have resolved them into motion. Take, for example, the theories of light, to which I have before alluded: one of these supposes light to be a peculiar rare matter, emitted from—*i.e.* put in *motion* by—luminous bodies; a second supposes that the specific matter is not emitted from luminous bodies, but is put into a state of vibration or undulation, *i.e. motion* by them; and thirdly, light may be regarded as an undulation or *motion* of ordinary matter, and propagated by undulations of air, glass, &c., as I have before stated. In all these hypotheses, matter and motion are the only conceptions. Nor, even if we accept terms derived from our own sensations, the which sensations themselves may be but modes of motion in the nervous filaments, can we find words to describe phenomena other than those expressive of matter and motion. We in vain struggle to escape from these

ideas ; if we ever do so, our mental powers must undergo a change of which at present we see no prospect.

If we apply to any other force the mode of reasoning which in the course of this Essay has been applied to heat, we shall arrive at the same conclusion, and see that a given source of power can, supposing it to be fully utilised in each case, yield no more by employing it as an exciter of one force than of another. Let us take electricity as an example. Suppose a pound of mercury at 400° be employed to produce a thermo-electric current, and the latter be in its turn employed to produce mechanical force ; if this latter force be greater than that which the direct effect of heat would produce, then it could by compression or friction raise the temperature of the mercury itself, or of a similar quantity equally heated, to a higher point than its original temperature, the 400° to 401°, for example, which is obviously impossible; nor, if we admit force to be indestructible, can it produce less than 400°, except by some portion of it being converted into another form or mode of force.

But as the mechanical effect here is produced through the medium of electricity, and the mechanical effect is definite, so the quantity of electricity producing it must be definite also, for unequal quantities of electricity could only produce an equal mechanical effect by a loss or gain of their own force into or out of nothing. The same reasoning will apply to the other forces, and will lead, it appears to me, necessarily and inevitably to the conclusion, that each force is definitely and equivalently convertible into any other, and that where experiment does not give the full equivalent, it is because the initial force has been dissipated, not lost, by conversion into other unrecognised forces. The equivalent is the limit never practically reached.

The great problem which remains to be solved, in regard to the correlation of physical forces, is this establishment of their equivalents of power, or their measurable relation to a given standard. The progress made in some of the branches of this enquiry has been already noticed. Viewed in their static relations, or in the conditions requisite for producing

equilibrium or quantitative equality of force, a remarkable relation between chemical affinity and heat is that discovered in many simple bodies by Dulong and Petit, and extended to compounds by Neumann and Avogadro. Their researches have shown that the specific heats of certain substances, when multiplied by their chemical equivalents, give a constant quantity as product—or, in other words, that the combining weights of such substances are those weights which require equal accessions or abstractions of heat, equally to raise or lower their temperature. To put the proposition more in accordance with the view we have taken of the nature of heat: each body has a power of communicating or receiving molecular repulsive power, exactly equal, weight for weight, to its chemical or combining power. For instance, the equivalent of lead is 104, of zinc 33, or in round numbers, as 3 to 1: these numbers are therefore inversely the exponents of their chemical power, three times as much lead as zinc being required to saturate the same quantity of an acid or substance combining with it; but their power of communicating or abstracting heat or repulsive power is precisely the same, for three times as much lead as zinc is required to produce the same amount of expansion or contraction in a given quantity of a third substance, such as water.

Again, a great number of bodies chemically combine in equal volumes, *i.e.* in the ratios of their specific gravities; but the specific gravities represent the attractive powers of the substances, or are the numerical exponents of the forces tending to produce motion in masses of matter of equal volume towards each other; while the chemical equivalents are the exponents of the affinities or tendencies of the molecules of dissimilar substances to combine and saturate each other; consequently, here we have in certain cases an equivalent relation between these two modes of force—gravitation and chemical attraction.

Were the above relations extended into an universal law, we should have the same numerical expression for the three forces of heat, gravity, and affinity; and as electricity and magnetism are quantitatively related to them, we should have a similar expression for these forces; but at present the

bodies in which this parity of force has been discovered, though in themselves numerous, are small compared with the exceptions, and, therefore, this point can only be indicated as promising a generalisation, should subsequent researches alter our knowledge as to the elements and combining equivalents of matter.

With regard to what may be called dynamic equivalents, *i.e.* the definite relation of the motive action of these varied forces upon equivalents of matter, the difficulty of establishing them is still greater. If the proposition which I stated at the commencement of this paper be correct, that motion may be subdivided or changed in character, so as to become heat, electricity, &c., it ought to follow that when we collect the dissipated and changed forces, and re-convert them, the initial motion, minus an infinitesimal quantity, affecting the same amount of matter with the same velocity, should be reproduced, and so of the changes in matter produced by the other forces; but the difficulties of proving the truth of this by experiment will, in many cases, be all but insuperable; we cannot imprison motion as we can matter, though we may to some extent restrain its direction.

The term perpetual motion, which I have not unfrequently employed in these pages, is itself equivocal. If the doctrines here advanced be well founded, all motion is, in one sense, perpetual. In masses whose motion is stopped by mutual concussion, heat or motion of the particles is generated; and thus the motion continues, so that if we could venture to extend such thoughts to the universe, we should assume the same amount of motion affecting the same amount of matter for ever. Where force is made to oppose force, and produce static equilibrium, the balance of pre-existing equilibrium is affected, and fresh motion is started equivalent to that which is withdrawn into a state of abeyance.

But the term perpetual motion is applied, in ordinary parlance (and in such sense I have used it), to a perpetual recurrent motion, *e.g.*, a weight which by its fall would turn a wheel, which wheel would, in its turn, raise the initial weight, and so on for ever, or until the material of which the machine is made be worn out. It is strange that to common appre-

hension the impossibility of this is not self-evident: if the initial weight is to be raised by the force it has itself generated, it must necessarily generate a force greater than that of its own weight or centripetal attraction; in other words, it must be capable of raising a weight heavier than itself; so that, setting aside the resistance of friction, &c., a weight, to produce perpetual recurrent motion, must be heavier than an equal weight of matter, in short, heavier than itself.

Suppose two equal weights at each end of an equi-armed lever, there is no motion; cut off a fraction of one of them, and it rises while the other falls. How, now, is the lesser weight to bring back the greater without any extraneous application of force? If, as is obvious, it cannot do so in this simple form of experiment, it is *à fortiori* more impossible if machinery be added, for increased resistances have then to be overcome. Can we again mend this by employing any other force? Suppose we employ electricity, the initial weight in descending turns a cylinder against a cushion, and so generates electricity; to make this force recurrent, the electricity so generated must, in its turn, raise the initial weight, or one heavier than it, *i.e.* the initial weight must, through the medium of the electricity it generates, raise a weight heavier than itself. The same problem, applied to any other forces, will involve the same absurdity; and yet, simple as the matter seems, the world is hardly yet disabused of an idea little removed from superstition.

But the importance of the deductions to be derived from the negation of perpetual motion seems scarcely to have impressed philosophers, and we only find here and there a scattered hint of the consequences necessarily resulting from that which to the thinking mind is a conviction. Some of these I have ventured to put forward in the present Essay, but many remain, and will crowd upon the mind of those who pursue the subject. Does not, for instance, the impossibility of perpetual motion, when thought out, involve the demonstration of the impossibility, to which I have previously alluded, of any event identically recurring?

The pendulum *in vacuo*, at each beat, leaves a portion of the force which started it, in the form of heat at its point of

suspension; this force, though ever existent, can never be restored in its integrity to the ball of the pendulum, for in the process of restoration it must affect other matter, and alter the condition of the universe. To restore the initial force in its integrity, everything as it existed at the moment of the first beat of the pendulum must be restored in its integrity; but how can this be?—for while the force was escaping from the pendulum by radiating heat from the point of suspension, surrounding matter has not stood still; the very attraction which caused the beat of the pendulum has changed in degree, for the pendulum is nearer to or farther from the sun, or from some planet or fixed star.

It might be an interesting and not profitless speculation to follow out as far as may be these and other consequences: it would, I believe, lead us to the conviction that the universe is ever changing, and that, notwithstanding secular recurrences which would, *primâ facie*, seem to replace matter in its original position, nothing, in fact, ever returns or can return to a state of existence identical with a previous state. But the field of enquiry is illimitable.

The inevitable dissipation or throwing off a portion of the initial force presents a great experimental difficulty in the way of establishing the equivalents of the various natural forces. In the steam-engine, for instance, the heat of the furnace not only expands the water and thereby produces the motion of the piston, but it also expands the iron of the boiler of the cylinder and all surrounding bodies. The force expended in expanding this iron to a very small extent is equal to that which expands the vapour to a very large extent: this expansion of the iron is capable, in its turn, of producing a great mechanical force, which is practically lost. Could all the force be applied to the vapour, an enormous addition of power would be gained for the same expenditure; and perhaps, even with our present means, more might be done in utilising the expansion of the iron.

Another great difficulty in experimentally ascertaining the dynamic equivalents of different forces arises from the effects of disruption, elongation, or the overcoming an existing force. Thus, when a part of the initial force employed is engaged in

twisting or tearing asunder matter previously held together by cohesive attraction, or in overcoming gravitation or inertia, the same amount of heat or electricity would not apparently be evolved as if such obstacle were non-existent, and the initial force were wholly employed in producing, not in opposing. There is great difficulty in devising experiments in which some portion of the force is not so employed.

The initial force, however, that has been employed for such disruption is not lost, as at the moment of disruption the bodies producing it fly off, and carry with them their force. Thus, let two weights be attached to a cord placed across a bar; when their force is sufficient to break the cord or the bar, the weights fall and strike the earth, making it vibrate, and so conveying away or continuing the force which the bar or cord had been acting on. If, instead of breaking a cord, the weights be employed to bend a bar, their gravitating force, instead of making the earth vibrate, produces heat in the bar, and so with whatever other force may be employed to produce effects of disruption, torsion, &c., so that, though difficult in practice, the numerical problem of the equivalent of the force is not theoretically irresolvable.

The voltaic battery affords us the best means of ascertaining the dynamic equivalents of different forces, and it is probable that by its aid the best theoretical and practical results will be ultimately attained.

In investigating the relation of the different forces, I have in turn taken each one as the initial force or starting-point, and endeavoured to show how the force thus arbitrarily selected could mediately or immediately produce and be merged into the others; but it will be obvious to those who have attentively considered the subject, and brought their minds into a general accordance with the views I have submitted to them, that no force can, strictly speaking, be initial, as there must be some anterior force which produced it: we cannot create force or motion any more than we can create matter.

Thus, to take an example previously noticed, and trace it backwards: the spark of light is produced by electricity, electricity by motion, and motion is produced by something

else, say a steam-engine—that is, by heat. This heat is produced by chemical affinity, *i.e.* the affinity of the carbon of the coal for the oxygen of the air: this carbon and this oxygen have been previously eliminated by the heat of the sun, or by other actions difficult to trace, but of the pre-existence of which we cannot doubt, and in which actions we should find the conjoint and alternating effects of heat, light, chemical affinity, &c. Thus, tracing any force backwards to its antecedents, we are merged in an infinity of changing forms of force; at some point we lose it, not because it has been in fact created at any definite point, but because it resolves itself into so many contributing forces, that the evidence of it is lost to our senses or powers of detection; just as, in following it forward into the effect it produces, it becomes, as I have before stated, so subdivided and dissipated as to be equally lost to our means of detection.

Can we, indeed, suggest a proposition, definitely conceivable by the mind, of force without antecedent force? I cannot, without calling for the interposition of creative power, any more than I can conceive the sudden appearance of a mass of matter come from nowhere, and formed from nothing. The impossibility, humanly speaking, of creating or annihilating matter, has long been admitted, though, perhaps, its distinct reception in philosophy may be set down to the overthrow of the doctrine of Phlogiston, and the reformation of chemistry at the time of Lavoisier. The reasons for the admission of a similar doctrine as to force appear to be equally strong. With regard to matter, there are many cases in which we never practically prove its cessation of existence, yet we do not the less believe in it: who, for instance, can trace, so as to re-weigh, the particles of iron worn off the tire of a carriage wheel? who can re-combine the particles of wax dissipated and chemically changed in the burning of a candle? By placing matter undergoing physical or chemical changes under special limiting circumstances, we may, indeed, acquire evidence of its continued existence, weight for weight—and so we may in some instances of force, as in definite electrolysis; indeed, the evidence we acquire of the continued existence of matter is by the continued exertion of the force it exercises,

as, when we weigh it, our evidence is the force of attraction; so, again, our evidence of force is the matter it acts upon. Thus, matter and force are correlates, in the strictest sense of the word; the conception of the existence of the one involves the conception of the existence of the other: the quantity of matter again, and the degree of force, involve conceptions of space and time. But to follow out these abstract relations would lead me too far into the alluring paths of metaphysical speculation.

That the theoretical portions of this Essay are open to objection I am fully conscious. I cannot, however, but think that the fair way to test a theory is to compare it with other theories, and to see whether, upon the whole, the balance of probability is in its favour. Were a theory open to no objection it would cease to be a theory, and would be called a law; and were we not to theorise, or to take generalised views of natural phenomena until those generalisations were sure and unobjectionable, science would be lost in a complex mass of unconnected observations, which would probably never disentangle themselves. Excess on either side is to be avoided; although we may often err on the side of hasty generalisation, we may equally err on the side of mere elaborate collection of observations, which, though sometimes leading to a valuable result, yet, when cumulated without a connecting link, frequently occasion a costly waste of time, and leave the subject to which they refer in greater obscurity than that in which it was involved at their commencement.

Collections of facts differ in importance, as do theories: the former, in many instances, derive their value from their capability of generalisation; while, conversely, theories are valuable as methods of co-ordinating given series of facts, and more valuable in proportion as they require fewer exceptions and fewer postulates. Facts may sometimes be as well explained by one view as by another, but without a theory they are unintelligible and incommunicable. Let us use our utmost effort to communicate a fact without using the language of theory, and we fail; theory is involved in all our expressions; the knowledge of bygone times is imported into succeeding times by terms involving theoretic conceptions. As the know-

ledge of any particular science developes itself, our views of it become more simple; hypotheses, or the introduction of supposititious views, are more and more dispensed with; words become applicable more directly to the phenomena, and, losing the hypothetic meaning which they necessarily possessed at their reception, acquire a secondary sense, which brings more immediately to our minds the facts of which they are indices. The scaffolding has served its purpose. The hypothesis fades away, and a theory, or generalised view of phenomena, more independent of supposition, but still full of gaps and difficulties, takes its place. This in its turn, should the science continue to progress, either gives place to a more simple and wider generalisation, or becomes, by the removal of objections, established as what is termed a law. Even in this more advanced stage words importing theory must be used, but phenomena are now intelligible and connected, though expressed by varied forms of speech.

To think on nature is to theorise; and difficult it is not to be led on by the continuities of natural phenomena to theories which appear forced and unintelligible to those who have not pursued the same path of thought; which, moreover, if allowed to gain an undue influence, seduce us from that truth which is the sole object of our pursuit.

Where to draw the line—where to say thus far we may go, and no farther, in any particular class of analogies or relations which Nature presents to us; how far to follow the progressive indications of thought, and where to resist its allurements, is a question of degree which must depend upon the judgment of each individual or of each class of thinkers; yet it is consolatory that thought is seldom expended in vain.

I have throughout endeavoured to discard the hypotheses of subtle or occult entities; if in this endeavour some of my views have been adopted upon insufficient data, I still hope that this Essay will not prove valueless.

The conviction that the so-called imponderables are modes of motion, will, at all events, lead the observer of natural phenomena to look for changes in these affections, wherever the intimate structure of matter is changed; and, conversely, to seek for changes in matter, either temporary or permanent,

whenever it is affected by these forces. I believe he will seldom do this in vain. It was not until I had long reflected on the subject that I ventured to publish my views; their publication may induce others to think on their subject-matter. They are not put forward with the same objects, nor do they aim at the same elaboration of detail, as memoirs on newly-discovered physical facts: they purport to be a method of mentally regarding known facts, some few of which I have myself made known on other occasions, but the great mass of which have been accumulated by the labours of others, and are admitted as established truths. Everyone has a right to view these facts through any medium he thinks fit to employ, but some theory must exist in the minds of those who reflect upon the many new phenomena which have recently, and more particularly during the present century, been discovered. It is by a generalised or connected view of past acquisitions in natural knowledge that deductions can best be drawn as to the probable character of the results to be anticipated. It is a great assistance in such investigations to be intimately convinced that no physical phenomenon can stand alone: each is inevitably connected with anterior changes, and as inevitably productive of consequential changes, each with the other, and all with time and space; and, either in tracing back these antecedents or following up their consequents, many new phenomena will be discovered, and many existing phenomena hitherto believed distinct will be connected and explained: explanation is, indeed, only relation to something more familiar, not more known—*i.e.* known as to causative or creative agencies. In all phenomena the more closely they are investigated the more are we convinced that, humanly speaking, neither matter nor force can be created or annihilated, and that an essential cause is unattainable.

CONTINUITY.

ADDRESS AS PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Nottingham, 1866. *Third Edition.*

If our rude predecessors, who at one time inhabited the caverns which surround this town, could rise from their graves and see it in its present state, it may be doubtful whether they would have sufficient knowledge to be surprised.

The machinery, almost resembling organic beings in delicacy of structure, by which are fabricated products of world-wide reputation, the powers of matter applied to give motion to that machinery, are so far removed from what must have been the conceptions of the semi-barbarians to whom I have alluded, that they could not look on them with intelligent wonder.

Yet this immense progress has all been effected step by step, now and then a little more rapidly than at other times; but, viewing the whole course of improvement, it has been gradual, though moving in an accelerated ratio. But it is not merely in those branches of natural knowledge which tend to improvements in economical arts and manufactures that science has made great progress. In the study of our own planet and the organic beings with which it is crowded, and in so much of the universe as vision, aided by the telescope, has brought within the area of observation, the present century has surpassed any antecedent period of equal duration.

It would be difficult to trace out all the causes which have led to the increase of observational and experimental knowledge.

Among the more thinking portion of mankind the gratifi-

cation felt by the discovery of new truths, the expansion of faculties, and extension of the boundaries of knowledge, have been doubtless a sufficient inducement to the study of nature; while, on more practical minds, the reality, the certainty, the progressive character of the acquisitions of natural science, and the enormously increased means which its applications give, have impressed its importance as a minister to daily wants and a contributor to ever-increasing material comforts, luxury, and power.

Though by no means the only one, yet an important cause of the rapid advance of science is the growth of associations for promoting the progress either of physical knowledge generally, or of special branches of it. Since the foundation of the Royal Society, now more than two centuries ago, a vast number of kindred societies have sprung up in this country and in Europe. The advantages conferred by these societies are manifold: they enable those who are devoted to scientific research to combine, compare, and check their observations; to assist, by the thoughts of several minds, the promotion of the enquiry undertaken; they contribute from a joint purse to such efforts as their members deem most worthy; they afford a means of submitting to a competent tribunal notices and memoirs, and of obtaining for their authors and others, by means of the discussions which ensue, information given by those best informed on the particular subject; they enable the author to judge whether it is worth his while to pursue the subjects he has brought forward; and they defray the expense of printing and publishing such researches as are thought meritorious.

These advantages, and others might be named, pertain to the Association the thirty-sixth meeting of which we are this evening assembled to inaugurate; but it has, from its intermittent and peripatetic character, advantages which belong to none of the societies which are fixed as to their locality.

Among these are the novelty and freshness of an annual meeting, which, while it brings together old members of the Association, many of whom only meet on this occasion, always adds a quota of new members, infusing fresh blood, and varying the social character of our meetings.

The visits of distinguished foreigners, whom we have previously known by reputation, is one of the most delightful and improving of the results. The wide field of enquiry, and the character of communications made to the Association, including all branches of natural knowledge, and varying from simple notices of an interesting observation or experiment, to the most intricate and refined expositions of scientific research, is another valuable characteristic.

Lastly, perhaps the greatest advantage resulting from the annual visits of this great parliament to new localities is, that while it imparts fresh local and general knowledge to the visitors, it leaves behind stimulating memories, which rouse into permanent activity dormant or timid minds—an effect which, so far from ceasing with the visit of the Association, frequently begins when that visit terminates.

Every votary of physical science must be anxious to see it recognised by those institutions of his country which can to the greatest degree promote its cultivation and reap from it the greatest benefit. You will probably agree with me that the principal educational establishments on the one hand, and on the other the Government, in many of its departments, are the institutions which may best fulfil these conditions. The more early the mind is trained to a pursuit of any kind, the deeper and more permanent are the impressions received, and the more service can be rendered by the students.

> 'Quo semel est imbuta recens, servabit odorem
> Testa diu.'

Little can be achieved in scientific research without an acquaintance with it in youth; you will rarely find an instance of a man who has attained any eminence in science who has not commenced its study at a very early period of life. Nothing, again, can tend more to the promotion of science than the exertions of those who have early acquired the ἦθος resulting from a scientific education. I desire to make no complaint of the tardiness with which science has been received at our public schools, and, with some exceptions, at our universities. These great establishments have their roots in historical periods, and long time and patient endeavour are

requisite before a new branch of thought can be grafted with success on a stem to which it is exotic. Nor should I ever wish to see the study of languages, of history, of all those refined associations which the past has transmitted to us, neglected; but there is room for both. It is sad to see the number of so-called educated men who, travelling by railway, voyaging by steamboat, consulting the almanac for the time of sunrise or full-moon, have not the most elementary knowledge of a steam-engine, a barometer, or a quadrant; and who will listen with a half-confessed faith to the most idle predictions as to weather or cometic influences, while they are in a state of crass ignorance as to the cause of the trade-winds, or the form of a comet's path. May we hope that the slight infiltration of scientific studies, now happily commenced, will extend till it occupies its fair space in the education of the young, and that those who may be able learnedly to discourse on the Eolic digamma will not be ashamed of knowing the principles on which the action of an air-pump, an electrical machine, or a telescope depend, and will not, as Bacon complained of his contemporaries, despise such knowledge as something mean and mechanical?

To assert that the great departments of Government should encourage physical science may appear a truism, and yet it is but of late that it has been seriously done. Now the habit of consulting men of science on important questions of national interest is becoming a recognised practice; and in a time, which may seem long to individuals, but is short in the history of a nation, a more definite sphere of usefulness for national purposes will, I have no doubt, be provided for those duly qualified men who may be content to give up the more tempting study of abstract science for that of its practical applications. In this respect the report of the Kew Committee for this year affords a subject of congratulation to those whom I have the honour to address. The Kew Observatory, the petted child of the British Association, may possibly become an important national establishment; and if so, while it will not, I trust, lose its character of a home for untrammelled physical research, it will have superadded some of the functions of the

Meteorological Department of the Board of Trade, with a staff of skilful and experienced observers.

This is one of the results which the general growth of science, and the labours of this Association in particular, have produced; but I do not propose on this occasion to recapitulate the special objects attained by the Association; this has been amply done by several of my predecessors; nor shall I confine my address to the progress made in physical science since the time when my most able and esteemed friend and predecessor addressed you at Birmingham. In the various reports and communications which will be read at your sections, details of every step which has been made in science since our last meeting will be brought to your notice, and I have no doubt fully and freely discussed.

I purpose, with your kind permission, to submit to you certain views of what has within a comparatively recent period been accomplished by science; what have been the steps leading to the attained results, and what, as far as we may fairly form an opinion, is the general character pervading modern discovery.

It seems to me that the object we have in view would be more nearly approached, by each President, chosen as they are in succession as representing different branches of science, giving on these occasions either an account of the progress of the particular branch of science he has cultivated, when that is not of a very limited and special character, or enouncing his own view of the general progress of science; and though this will necessarily involve much that belongs to recent years, the confining a President to a mere *résumé* of what has taken place since our last meeting would, I venture with diffidence to think, limit his means of usefulness, and render his discourse rather an annual register than an instructive essay.

I need not dwell on the commonplace but yet important topics of the material advantages resulting from the application of science; I will address myself to what, in my humble judgment, are the lessons we have learned, and the probable prospects of improved natural knowledge.

One word will give you the key to what I am about to

discourse on ; that word is *continuity*—no new word, and used in no new sense, but perhaps applied more generally than it has hitherto been. We shall see, unless I am much mistaken, that the development of observational, experimental, and even deductive knowledge is either attained by steps extremely small and forming really a continuous march ; or, when distinct results apparently separate from any co-ordinate phenomena have been attained, that then, by the subsequent progress of science, intermediate links have been discovered uniting the apparently segregated instances with other more familiar phenomena. We shall see that the more we investigate, the more we find that in existing phenomena graduation from the like to the seemingly unlike prevails, and in the changes which take place in time gradual progress is, and apparently must be, the course of nature.

Let me now endeavour to apply this view to the recent progress of some of the more prominent branches of science.

In Astronomy, from the time when the earth was considered a flat plain bounded by a flat ocean—when the sun, moon, and stars were regarded as lamps to illuminate this plain—each successive discovery has brought with it similitudes and analogies between this earth and many of the objects of the universe, with which our senses, aided by instruments, have made us acquainted. I pass, of course, over those discoveries which have established the Copernican system as applied to our sun, its attendant planets, and their satellites. The proofs, however, that gravitation is not confined to our solar system, but pervades the universe, have received many confirmations by the labours of members of this Association. I may name those who have held the office of President, Lord Rosse, Lord Wrottesley, and Sir J. Herschel, the two latter having devoted special attention to the orbits of double stars, the former to those probably more recent systems called nebulæ. Double stars seem to be orbs analogous to our own sun, and revolving round their common centre of gravity in a conic-section curve, as do the planets with which we are more intimately acquainted ; but the nebulæ present more difficulty, and some doubt has been expressed whether gravitation, as we understand it, acts with those bodies (at least those exhi-

biting a spiral form) as it does with us ; possibly some other modifying influence may exist, our present ignorance of which gives rise to the apparent difficulty. There is, however, another class of observations quite recent in its importance, and which has formed a special subject of contribution to the reports and transactions of this Association ; I allude to those on Meteorites, at which our lamented member, and to many of us our valued friend, Prof. Baden Powell, assiduously laboured, for investigations into which a Committee of this Association is formed ; and a series of star-charts for enabling observers of shooting-stars to record their observations was laid before the last meeting of the Association by Mr. Glaisher.

It would occupy too much of your time to detail the efforts of Bessel, Schwinke, the late Sir J. Lubbock, and others, as applied to the formation of star-charts for aiding the observation of meteorities which Mr. Alexander Herschel, Mr. Brayley, Mr. Sorby, and others are now studying.

Dr. Olmsted explained the appearance of a point from which the lines of flight of meteors seem to radiate, as being the perspective vanishing-point of their parallel or nearly parallel courses, appearing divergent to an observer on the earth as they approach it. The uniformity of position of these radiant points, the many corroborative observations on the direction, the distances, and the velocities of these bodies, the circumstance that their paths intersect the earth's orbit at certain definite periods, and the total failure of all other theories which have been advanced, while there is no substantial objection to this, afford evidence almost amounting to proof that these are cosmical bodies moving in the interplanetary space by gravitation around the sun, and some perhaps around planets. This view gives us a new element of continuity. The universe would thus appear not to have the extent of empty space formerly attributed to it, but to be studded between the larger and more visible masses with smaller planets, if the term be permitted to be applied to meteorites.

Observations are now made at the periods at which meteors appear in greatest numbers—at Greenwich, by Mr. Glaisher ; at Cambridge, by Prof. Adams ; and at Hawkhurst, by Mr. Alexander Herschel ; and every preparation is made

to secure as much accuracy as can, in the present state of knowledge, be attained by such observations.

The number of known asteroids, or bodies of a smaller size than what are termed the ancient planets, has been so increased by numerous discoveries, that instead of seven we now count eighty-eight as the number of recognised planets, a field of discovery with which the name of Hind will be ever associated.

The smallest of these is only twenty or thirty miles in diameter, indeed cannot be accurately measured; and if we were to apply the same scrutiny to other parts of the heavens as has been applied to the zone between Mars and Jupiter, it is no far-fetched speculation to suppose that, in addition to asteroids and meteorites, many other bodies exist, so that the space occupied by our solar system may be dotted up with planetary bodies varying in size from that of Jupiter (1,240 times larger in volume than the earth) to that of a cannon-ball or even a pistol-bullet.

The researches of Leverrier on the intra-Mercurial planets come in aid of these views; and another half-century may, and not improbably will, enable us to ascertain that the now seemingly vacant interplanetary spaces are occupied by smaller bodies, which have hitherto escaped observation, just as the asteroids had until the time of Olbers and Piazzi. But the evidence of continuity as pervading the universe does not stop at the results of telescopic observation; chemistry and physical optics bring us new proofs. Those meteoric bodies which have from time to time come so far within reach of the earth's attraction as to fall upon its surface, give on analysis metals and oxides similar to those which belong to the structure of the earth—they come as travellers bringing specimens of minerals from extra-terrestrial regions.

In a series of papers recently communicated to the French Academy, M. Daubrée has discussed the chemical and mineralogical character of meteorites as compared with the rocks of the earth. He finds that the similarity of terrestrial rocks to meteorites increases as we penetrate deeper into the earth's crust, and that some of the deep-seated minerals have a composition and characteristics almost identical with meteorites

[olivine, herzolite, and serpentine, for instance, closely resemble them]; that as we approach the surface rocks having similar components with meteorites are found, but in a state of oxidation, which necessarily much modifies their mineral character, and which, by involving secondary oxygenised compounds, must also change their chemical constitution. By experiments he has succeeded in forming from terrestrial rocks substances very much resembling meteorites. Thus close relationship, though by no means identity of composition, is established between this earth and those wanderers from remote regions, some evidence, though at present incomplete, of a common origin.

Surprise has often been expressed that, while the mean specific gravity of this globe is from five to six times that of water, the mean specific gravity of its crust is barely half as great. It has long seemed to me that there is no ground for wonder here. The exterior of our planet is to a considerable depth oxidated; the interior is in all probability free from oxygen, and whatever bodies exist there are in a reduced or deoxidated state; if so, the specific gravity of most of them must necessarily be higher than that of their oxides or chlorides, &c.; we find, moreover, that many of the deep-seated minerals have a higher specific gravity than the average of those on the surface; olivine, for instance, has a specific gravity of 3·3. There is therefore no *à priori* improbability that the mean specific gravity of the earth should notably exceed that of its surface; and if we go farther, and suppose the interior of the earth to be formed of the same ingredients as the exterior, minus oxygen, chlorine, bromine, &c., a specific gravity of 5 to 6 would not be an unlikely one. Many of the elementary bodies entering largely into the formation of the earth's crust are as light or lighter than water—for instance, potassium, sodium, &c.; others, such as sulphur, silicon, aluminium, have from two to three times its specific gravity; others, again, as iron, copper, zinc, tin, seven to nine times; while others, lead, gold, platinum, &c., are much more dense; but, speaking generally, the more dense are the least numerous. There seems no improbability in a mixture of such substances producing a mean specific gravity of from 5 to 6, although it by no means

follows, indeed the probability is rather the other way, that the proportions of the substances in the interior of the earth are the same as on the exterior. It might be worth the labour to ascertain the mean specific gravity of all the known minerals on the earth's surface, averaging them on the ratios in which, as far as our knowledge goes, they quantitatively exist, and assuming them to exist without the oxygen, chlorine, &c., with which they are, with some rare exceptions, invariably combined on the surface of the earth: great assistance to the knowledge of the probable constitution of the earth might be derived from such an investigation.

While chemistry, analytic and synthetic, thus aids us in ascertaining the relationship of our planet to meteorites, its relation in composition to other planets, to the sun, and to more distant suns and systems, is aided by another science, viz. optics.

That light passing from one transparent medium to another should carry with it evidence of the source from which it emanates, would, until lately, have seemed an extravagant supposition; but probably (could we read it) everything contains in itself a large portion of its own history.

I need not detail to you the discoveries of Kirchhoff, Bunsen, Miller, Huggins, and others; they have been dilated on by my predecessor. Assuming that spectrum analysis is a reliable indication of the presence of given substances by the position of transverse bright lines exhibited when they are burnt, and of transverse dark lines when light is transmitted through their vapours, though Plücker has shown that with some substances these lines vary with temperature, the point of importance in the view I am presenting to you is, that while what may be called comparatively neighbouring cosmical bodies exhibit lines identical with many of those shown by the components of this planet, as we proceed to the more distant appearances of the nebulæ we get but one or two of such lines, and we get one or two new lines not yet identified with any known to be produced by substances on this globe.

Within the last year Mr. Huggins has added to his former researches observations on the spectrum of a comet (comet 1 of 1866), the nucleus of which shows but one bright line, while

the spectrum formed by the light of the coma is continuous, seeming to show that the nucleus is gaseous, while the coma would consist of matter in a state of minute division shining by reflected light: whether this be solid, liquid, or gaseous is doubtful, but the author thinks it is in a condition analogous to that of fog or cloud. The position in the spectrum of the bright line furnished by the nucleus is the same as that of nitrogen, which line is also shown in some of the nebulæ.

But the most remarkable achievement by spectrum analysis is the record of observations on a temporary star which has shone forth this year in the constellation of the Northern Crown, about a degree S.E. of the star ε. When it was first seen, May 12, it was nearly equal in brilliancy to a star of the second magnitude; when observed by Mr. Huggins and Dr. Miller, May 16, it was reduced to the third or fourth magnitude. Examined by these observers with the spectroscope, it gave a spectrum which they state was unlike that of any celestial body they had examined.

The light was compound, and had emanated from two different sources. One spectrum was analogous to that of the sun, viz. formed by the light of an incandescent solid or liquid photosphere which had suffered absorption by the vapours of an envelope cooler than itself. The second spectrum consisted of a few bright lines, which indicated that the light by which it was formed was emitted by matter in a state of luminous gas. The observers consider that, from the position of two of the bright lines, the gas must probably be hydrogen, and from their brilliancy compared with the light of the photosphere the gas must have been at a very high temperature. They imagine the phenomena to result from the burning of hydrogen with some other element, and that from the resulting temperature the photosphere is heated to incandescence.

There is strong reason to believe that this star is one previously seen by Argelander and Sir J. Herschel, and that it is a variable star of long or irregular period; it is also notable that some of its spectrum lines correspond with those of several variable stars. The time of its appearance was too short for any attempt to ascertain its parallax; it would have been important if it could even have been established that it is not a

near neighbour, as the magnitude of such a phenomenon must be inverse to its distance. I forbear to add any speculations as to the cause of this most singular phenomenon. However imperfect the knowledge given us by these observations, it is a great triumph to have caught this fleeting object, and obtained permanent records for the use of future observers.

It would seem as if the phenomenon of gradual change obtained towards the remotest objects with which we are at present acquainted, and that the farther we penetrate into space the more unlike to those we are acquainted with become the objects of our examination—sun, planets, meteorites, earth, similarly though not identically constituted, stars differing from each other and from our system, and nebulæ more remote in space and differing more in their characters and constitution.

While we can thus to some extent investigate the physical constitution of the most remote visible substances, may we not hope that some farther insight as to the constitution of the nearest, viz. our own satellite, may be given us by this class of researches? The question whether the moon possesses any atmosphere may still be regarded as unsolved. If there be any, it must be exceedingly small in quantity and highly attenuated. Calculations, made from occultations of stars, on the apparent differences of the semi-diameter of the bright and dark moon, give an amount of difference which might indicate a minute atmosphere, but which Mr. Airy attributes to irradiation.

Supposing the moon to be constituted of similar materials to the earth, it must be, to say the least, doubtful whether there is oxygen enough to oxidate the metals of which she is composed; and if not, the surface which we see must be metallic, or nearly so. The appearance of her craters is not unlike that seen on the surface of some metals, such as bismuth, or, according to Professor Phillips, silver, when cooling from fusion, and just previous to solidifying; and it might be a fair subject of enquiry whether, if there be any coating of oxide on the surface, it may not be so thin as not to disguise the form of the congealed metallic masses, as they may have set in cooling from igneous fusion. M. Chacornac's recent observations lead him to suppose that many of the lunar

craters were the result of a single explosion, which raised the surface as a bubble and deposited its *débris* around the orifice of eruption.

The eruptions on the surface of the moon clearly did not take place at one period only, for at many parts of the disk craters may be seen encroaching on and disfiguring more ancient ones, sometimes to the extent of three or four successive displacements. Two important questions might, it seems to me, be solved by an attentive examination of such portions of the moon. By observing carefully with the most powerful telescopes the character of the ridges thus successively formed, the successive states of the lunar surface at different epochs might be elucidated ; and, secondly, as on the earth we should look for actual volcanic action at those points where recent eruptions have taken place, so on the moon the more recently active points being ascertained by the successive displacement of anterior formations, it is these points which should be examined for existing disruptive disturbances. Metius and Fabricius might be cited as points of this character, having been found by M. Chacornac to present successive displacements, and to be perforated by numerous channels or cavities. M. Chacornac considers that the seas, as they are called, or smoother portions of the lunar surface, have at some time made inroads on anteriorly formed craters ; if so, a large portion of the surface of the moon must have been in a fused, liquid, semi-liquid, or alluvial state long after the solidifying of other portions of it. It would be difficult to suppose that this state was one of igneous fusion, for this could hardly exist over a large part of the surface without melting up the remaining parts ; on the other hand, the total absence of any signs of water, and of any atmosphere, or, if any there be, it is most attenuated, would make it equally difficult to account for a large diluvial formation.

Some substances, like mercury on this earth, might have remained liquid after others had solidified by the cooling of the planet ; but the problem is one which needs more examination and study before any positive opinion can be pronounced.

I cannot pass from the subject of lunar physics without recording the obligation we are under to our late President

for his most valuable observations and for his exertions in organising a band of observers devoted to the examination of our nearest celestial neighbour; and to Mr. Nasmyth and Mr. De la Rue for their important graphical and photographical contributions to this subject. The granular character of the sun's surface observed by Mr. Nasmyth in 1860 is also a discovery which ought not to be passed over in silence.

Before quitting the subject of Astronomy I cannot avoid expressing a feeling of disappointment that the achromatic telescope, which has rendered such notable service to this science, still retains in practice the great defect which was known a century ago, at the time of Hall and Dollond, namely, the inaccuracy of definition arising from what was termed the irrationality of the spectrum, or the incommensurate divisions of the spectra formed by flint and crown glass.

The beautiful results obtained by Blair have remained inoperative from the circumstance that evaporable liquids being employed between the lenses, a want of permanent uniformity in the instrument was experienced; and notwithstanding the high degree of perfection to which the grinding and polishing object-glasses has been brought by Clarke, Cooke, and Mertz, notwithstanding the greatly improved instrumental manufacture, the defect to which I have adverted remains unremedied and an eyesore to the observer with the refracting telescope.

We have now a large variety of different kinds of glass formed from different metallic oxides. A list of many such was given by M. Jacquelain a few years back. The last specimen which I have seen is a heavy high refracting glass formed from the metal thallium by M. Lamy. Among all these, could not two or three be selected which, having appropriate refracting and dispersing powers, would have the coloured spaces of their respective spectra, if not absolutely in the same proportions, at all events much more nearly so than those of flint and crown glass? Could not, again, oily or resinous substances having much action on the more refrangible rays of the spectrum, such as castor-oil, Canada-balsam, &c., be made use of in combination with glass lenses to reduce if not annihilate this signal defect? I have succeeded, to some

extent, in experiments with this class of substances. It is not a problem to the solution of which there seems any insuperable difficulty ; the reason why it has not been solved is, I incline to think, that the great practical opticians have no time at their disposal to devote to long tentative experiments and calculations; and on the other hand the theoretic opticians have not the machinery and the skill in manipulation requisite to give the appropriate degree of excellence to the materials with which they experiment; yet the result is worth labouring for, as, could the defect be remedied, the refracting telescope would make nearly as great an advance upon its present state as the achromatic did on the single lens refractor.

While gravitation, physical constitution, and chemical analysis by the spectrum show us that matter has similar characteristics in other worlds than our own, when we pass to the consideration of those other attributes of matter which were at one time supposed to be peculiar kinds of matter itself, or, as they were called, imponderables, but which are now generally, if not universally, recognised as forces or modes of motion, we find the evidence of continuity still stronger.

When all that was known of magnetism was that a piece of steel rubbed against a particular mineral had the power of attracting iron, and, if freely suspended, of arranging itself nearly in a line with the earth's meridian, it seemed an exceptional phenomenon. When it was observed that amber, if rubbed, had the temporary power of attracting light bodies, this also seemed something peculiar and anomalous. What are now magnetism and electricity as known to us ? Forces so universal, so apparently connected with matter, as to become two of its invariable attributes, and that to speak of matter not being capable of being affected by these forces would seem almost as extravagant as to speak of matter not being affected by gravitation.

So with light, heat, and chemical affinity, not merely is every form of matter with which we are acquainted capable of manifesting all these modes of force, but so-called matter supposed incapable of such manifestations would to most minds cease to be matter.

Further than this it seems to me (though, as I have taken

an active part for many years, dating from a quarter of a century, in promoting this view, I may not be considered an impartial judge) that it is now proved that all these forces are so invariably connected *inter se* and with motion as to be regarded as modifications of each other, and as resolving themselves objectively into motion, and subjectively into that something which is supposed to produce or resist motion, and which we call force.

I may perhaps be permitted to recall a forgotten experiment, which nearly a quarter of a century ago I showed at the London Institution, an experiment simple enough in itself, but which then seemed to me important from the consequences to be deduced from it, and the importance of which will be much better appreciated now than then.

A train of multiplying wheels ended with a small metallic wheel, which, when the train was put in motion, revolved with extreme rapidity against the periphery of the next wheel, a wooden one. In the metallic wheel was placed a small piece of phosphorus, and as long as the wheels revolved the phosphorus remained unchanged; but the moment the last wheel was stopped, by moving a small lever attached to it, the phosphorus burst into flame. My object was to show that while motion of the mass continued heat was not generated, but that when this was arrested, the force continuing to operate, the motion of the mass became heat in the particles. The experiment differed from that of Rumford's cannon-boring and Davy's friction of ice in showing that there was no heat while the motion was unresisted, but that the heat was dependent on the motion being impeded or arrested. We have now become so accustomed to this view, that whenever we find motion resisted we look to heat, electricity, or some other force as the necessary and inevitable result.

It would be out of place here, and treating of matters too familiar to the bulk of my audience, to trace how, by the labours of Oersted, Seebeck, Faraday, Talbot, Daguerre, and others, materials have been provided for the generalisation now known as the correlation of forces or conservation of energy; while Davy, Rumford, Seguin, Mayer, Joule, Helmholtz, Thomson, and others (among whom I would not name

myself, were it not that I may be misunderstood and supposed to have abandoned all claim to a share in the initiation of this, as I believe, important generalisation) have carried on the work; and how, sometimes by independent and, as is commonly the case, nearly simultaneous deductions, sometimes by progressive and accumulated discoveries, the doctrine of the reciprocal interaction, of the quantitative relation, and of the necessary dependence of all the forces, has, I think I may venture to say, been established.

If magnetism be, as it is proved to be, connected with the other forces or affections of matter, if electrical currents always produce, as they are proved to do, lines of magnetic force at right angles to their lines of action, magnetism must be cosmical, for where there is heat and light there is electricity, and consequently magnetism. Magnetism, then, must be cosmical and not merely terrestrial. Could we trace magnetism in other planets and suns as a force manifested in axial or meridional lines, *i.e.* in lines cutting at right angles the curves formed by their rotation round an axis, it would be a great step; but it is one hitherto unaccomplished. The apparent coincidences between the maxima and minima of solar spots, and the decennial or undecennial periods of terrestrial magnetic intensity, though only empirical at present, might tend to lead us to a knowledge of the connection we are seeking; and Sir Edward Sabine considers that an additional epoch of coincidence has arrived, making the fourth decennial period; but some doubt is thrown upon these coincidences by the magnetic observations made at Greenwich Observatory. In a paper published in the 'Transactions of the Royal Society,' 1863, the Astronomer-Royal says, speaking of results extending over seventeen years, there is no appearance of decennial cycle in the recurrence of great magnetic disturbances; and Mr. Glaisher last year, in the physical section of this Association, stated that after persevering examination he had been unable to trace any connection between the magnetism of the earth and the spots on the sun.

Mr. Airy, however, in a more recent paper, suggests that currents of magnetic force having reference to the solar hour are detected, and seem to produce vortices or circular disturb-

ances, and he invites further co-operative observation on the subject, one of the highest interest, but at present remaining in great obscurity.

One of the most startling suggestions as to the consequence resulting from the dynamical theory of heat is that made by Mayer, that by the loss of *vis viva* occasioned by friction of the tidal waves, as well as by their forming, as it were, a drag upon the earth's rotatory movement, the velocity of the earth's rotation must be gradually diminishing, and that thus, unless some undiscovered compensatory action exist, this rotation must ultimately cease and changes hardly calculable take place in the solar system.

M. Delaunay considers part of the acceleration of the moon's mean motion, which is not at present accounted for by planetary disturbances, to be due to the gradual retardation of the earth's rotation; to which view, after an elaborate investigation, the Astronomer-Royal has given his assent.

Another most interesting speculation of Mayer is that with which you are familiar, viz. that the heat of the sun is occasioned by friction or percussion of meteorites falling upon it: there are some difficulties, not perhaps insuperable, in this theory. Supposing such cosmical bodies to exist in sufficient numbers, they would, as they revolve round the sun, fall into it, not as an aërolite falls upon the earth, directly by an intersection of orbits, but by the gradual reduction in size of the orbits, occasioned by a resisting medium; some portion of motive force would be lost, and heat generated in space by friction against such medium. When these bodies arrive at the sun they would, assuming them, like the planets, to have revolved in the same direction, all impinge in a definite direction, and we might expect to see some symptoms of their action in the sun's photosphere; but though this is in a constant state of motion, and the direction of these movements has been carefully investigated by Mr. Carrington and others, no such general direction is detected; and M. Faye, who some time ago wrote a paper pointing out many objections to the theory of solar heat being produced by the fall of meteoric bodies into the sun, has recently investigated the proper motions of sun-spots, and believes he has removed certain apparent anomalies and re-

duced their motions to a certain regularity in the motion of the photosphere, attributable to some general action arising from the internal mass of the sun.

It might be expected that comets, bodies so light and so easily deflected from their course, would show some symptoms of being acted on by gravitation, were such a number of bodies to exist in or near their paths as are pre-supposed in this theory of solar heat. The sun may, however, meet supplies of meteoric bodies and condense gaseous matter as it travels in space (as the sun is now fairly proved to do), and so heat may be produced.

Assuming the undulatory theory of light to be true, and that the motion which constitutes light is transmitted across the interplanetary spaces by a highly elastic ether, then, unless this motion is confined to one direction, unless there be no interference, unless there be no viscosity, as it is now termed, in the medium, and, consequently, no friction, light must lose something in its progress from distant luminous bodies, that is to say, must lose something as light; for, as all reflecting minds are now convinced that force cannot be annihilated, the force is not lost, but its mode of action is changed. If light, then, is lost as light (and the observations of Struve seem to show this to be so, that in fact a star may be so far distant that it can never be seen, in consequence of its luminous undulations becoming extinct), what becomes of the transmitted force lost as light, but existing in some other form? So with heat: our sun, our earth, and planets are constantly radiating heat into space, so in all probability are the other suns, the stars, and their attendant planets. What becomes of the heat thus radiated into space? If the universe have no limit—and it is difficult to conceive one—heat and light should be everywhere uniform; and night would be as light and as warm as day. What becomes of the enormous force thus apparently non-recurrent in the same form? Does it return as palpable motion? Does it move or contribute to move suns and planets? Can it be conceived as a force similar to that which Newton speculated on as universally repulsive and capable of being substituted for universal attraction? We are in no position at present to answer such questions as these; but I know of

no problem in celestial dynamics more deeply interesting than this, and we may be no farther removed from its solution than the predecessors of Newton were from the simple dynamical relation of matter to matter which his potent intellect detected and demonstrated.

Passing from extra-terrestrial theories to the narrower field of molecular physics, we find the doctrine of correlation of forces steadily making its way. In the Bakerian Lecture for 1863 Mr. Sorby shows, not perhaps a direct correlation of mechanical and chemical forces, but that when, either by solution or by chemical action, a change in volume of the resulting substance as compared with that of its separate constituents is effected, the action of pressure retards or promotes the change, according as the substance formed would occupy a larger or smaller space than that occupied by its separate constituents; the application of these experiments to geological enquiries as to subterranean changes which may have taken place under great pressure is obvious, and we may expect to form compounds under artificial compression which cannot be found under normal pressure.

In a practical point of view the power of converting one mode of force into another is of the highest importance; and with reference to a subject which at present, somewhat prematurely, perhaps, occupies men's minds, viz. the prospective exhaustion of our coal-fields, there is every encouragement to be derived from the knowledge that we can at will produce heat by the expenditure of other forces; but, more than that, we may probably be enabled to absorb or store up, as it were, diffused energy—for instance, Berthelot has found that the potential energy, as it is termed, of formate of potash is much greater than that of its proximate constituents, caustic potash and carbonic oxide. This change may take place spontaneously and at ordinary temperatures, and by such change carbonic oxide becomes, so to speak, re-invested with the amount of potential energy which its carbon possessed before uniting with oxygen, or, in other words, the carbonic oxide is raised as a force-possessor to the place of carbon by the direct absorption or conversion of heat from surrounding matter.

This may bear on certain recent speculations on the dissipation of force by heat radiation.

Here also we have as to force-absorption, an analogous result to that of the formation of coal from carbonic acid and water; and though this is a mere illustration, and cannot be expected to become economical on a large scale, still it and similar examples may calm apprehension as to future means of supplying heat, should our present fuel become exhausted. As the sun's force, spent in times long past, is now returned to us from the coal which was formed by its light and heat, so the sun's rays, which are daily wasted, as far as we are concerned, on the sandy deserts of Africa, may hereafter by chemical or mechanical means, be made to light and warm the habitations of the denizens of colder regions. The tidal wave is, again, a large reservoir of force hitherto almost unused.

The valuable researches of Professor Tyndall on radiant heat afford many instances of the power of localising, if the term be permitted, heat which would otherwise be dissipated.

The discoveries of Graham, by which atmospheric air, drawn through films of caoutchouc, leaves behind half its nitrogen, or, in other words, becomes richer by half in oxygen, and hence has a much increased potential energy, not only show a most remarkable instance of physical molecular action merging into chemical, but afford us indications of means of storing up force, much of the force used in working the aspirator being capable at any period, however remote, of being evolved by burning the oxygen with a combustible.

What changes may take place in our modes of applying force before the coal-fields are exhausted it is impossible to predict. Even guesses at the probable period of their exhaustion are uncertain. There is a tendency to substitute for smelting in metallurgic processes, liquid chemical action, which of course has the effect of saving fuel; and the waste of fuel in ordinary operations is enormous, and can be much economised by already known processes. It is true that we are, at present, far from seeing a practical mode of replacing that granary of force, the coal-fields; but we may with confidence

rely on invention being in this case, as in others, born of necessity, when the necessity arises.

I will not further pursue this subject. At a time when science and civilisation cannot prevent large tracts of country being irrigated by human blood in order to gratify the ambition of a few restless men, it seems an over-refined sensibility to occupy ourselves with providing means for our descendants in the tenth generation to warm their dwellings or propel their locomotives.

Two very remarkable applications of the convertibility of force have been recently attained by the experiments of Mr. Wilde and Mr. Holz; the former finds that, by conveying electricity from the coils of a magneto-electric machine to an electro-magnet, a considerable increase of electrical power may be attained, and by applying this as a magneto-electric machine to a second, and this in turn to a third electro-magnetic apparatus, the force is largely augmented. Of course, to produce this increase, more mechanical force must be used at each step to work the magneto-electric machines; but provided this be supplied, there hardly seems a limit to the extent to which mechanical may be converted into electrical force.

Mr. Holz has contrived a Franklinic electrical machine, in which a similar principle is manifested. A varnished glass plate is made to revolve in close proximity to another plate having two or more pieces of card attached, which are electrified by a bit of rubbed glass or ebonite; the moment this is effected, a resistance is felt by the operator who turns the handle of the machine, and the slight temporary electrisation of the card converts into a continuous flood of intense electricity the force supplied by the arm of the operator.

These results offer great promise of extended application; they show that, by a mere formal disposition of matter, one force may be converted into another, and that not to the limited extent hitherto attained, but to an extent co-ordinate, or nearly so, with the increased initial force, so that, by a mere change in the arrangement of apparatus, a means of absorbing and again eliminating in a new form a given force may be obtained to an indefinite extent. As we may, in a not very distant future, need, for the daily uses of mankind, heat, light,

and mechanical force, and find our present resources exhausted, the more we can invent new modes of conversion of forces, the more prospect we have of practically supplying such want. It is but a month from this time that the greatest triumph of force-conversion has been attained. The chemical action generated by a little salt-water on a few pieces of zinc will now enable us to converse with inhabitants of the opposite hemisphere of this planet, and

'Put a girdle round about the earth in forty minutes.'

The Atlantic Telegraph is an accomplished fact.

In Physiology very considerable strides are being made by studying the relation of organised bodies to external forces; and this branch of enquiry has been promoted by the labours of Carpenter, Bence Jones, Playfair, E. Smith, Frankland, and others. Vegetables acted on by light and heat decompose water, ammonia, and carbonic acid, and transform them into, among other substances, oxalate of lime, lactic acid, starch, sugar, stearine, urea, and ultimately albumen; while the living animal reverses the process, as does vegetable decay, and produces from albumen, urea, stearine, sugar, starch, lactic acid, oxalate of lime, and ultimately ammonia, water, and carbonic acid.

As moreover, heat and light are absorbed or converted in forming the synthetic processes going on in the vegetable, so conversely heat and sometimes light is given off by the living animal; but it must not be forgotten that the line of demarcation between a vegetable and an animal is difficult to draw, that there are no single attributes which are peculiar to either, and that it is only by a number of characteristics that either can be defined.

The series of processes above given may be simulated by the chemist in his laboratory; and the amount of labour which a man has undergone in the course of twenty-four hours may be approximately arrived at by an examination of the chemical changes which have taken place in his body; changed forms in matter indicating the anterior exercise of dynamical force. That muscular action is produced or supported by

chemical change would probably now be a generally accepted doctrine; but while many have thought that muscular power is derived from the oxidation of albuminous or nitrogenised substances, several recent researches seem to show that the latter is rather an accompaniment than a cause of the former, and that it is by the oxidation of carbon and hydrogen compounds that muscular force is supplied. Traube has been prominent in advancing this view, and experiments detailed in a paper published this year by two Swiss professors, Drs. Fick and Wislicenus, which were made by and upon themselves in an ascent of the Faulhorn, have gone far to confirm it. Having fed themselves, before and during the ascent, upon starch, fat, and sugar, avoiding all nitrogenised compounds, they found that the consumption of such food was amply sufficient to supply the force necessary for their expedition, and that they felt no exhaustion. By appropriate chemical examination they ascertained that there was no notable increase in the oxidation of the nitrogenised constituents of the body. After calculating the mechanical equivalents of the combustion effected, they state, as their first conclusion, that 'the burning of protein substances cannot be the only source of muscular power, for we have here two cases in which men performed more measurable work than the equivalent of the amount of heat which, taken at a most absurdly high figure, could be calculated to result from the burning of the albumen.'

They further go on to state that, so far from the oxidation of albuminous substances being the only source of muscular power, 'the substances by the burning of which force is generated in the muscles, are not the albuminous constituents of those tissues, but non-nitrogenous substances, either fats or hydrates of carbon,' and that the burning of albumen is not in any way concerned in the production of muscular power.

We must not confuse the question of the food which forms and repairs muscle and gives permanent capability of muscular force with that which supplies the requisites for temporary activity; no doubt the carnivora are the most powerfully constituted animals, but the chamois, gazelle, &c., have great temporary capacity for muscular exertion, though their food is

vegetable; for concentrated and sustained energy, however, they do not equal the carnivora; and with the domestic graminivora we certainly find that they are capable of performing more continuous work when supplied with those vegetables which contain the greatest quantity of nitrogen.

These and many similar classes of research show that in chemical enquiries, as in other branches of science, we are gradually relieving ourselves of hypothetical existences, which certainly had the advantage that they might be varied to suit the requirements of the theorist.

Phlogiston, as Lavoisier said with a sneer, was sometimes heavy, sometimes light; sometimes fire in a free state, sometimes combined; sometimes passing through glass vessels, sometimes retained by them; which by its protean changes explained causticity and non-causticity, transparency and opacity, colours and their absence. As phlogiston and similar creations of the mind have passed away, so with hypothetic fluids, imponderable matters, specific ethers, and other inventions of entities made to vary according to the requirements of the theorist, I believe the day is approaching when these will be dispensed with, and when the two fundamental conceptions of matter and motion will be found sufficient to explain physical phenomena.

The facts made known to us by geological enquiries, while on the one hand they afford striking evidence of continuity, on the other, by the breaks in the record, may be used as arguments against it. The great question once was, whether these chasms represent sudden changes in the formation of the earth's crust, or whether they arise from dislocations occasioned since the original deposition of strata or from gradual shifting of the areas of submergence. Few geologists of the present day would, I imagine, not adopt the latter alternatives. Then comes a second question, whether, when the geological formation is of a continuous character, the different characters of the fossils represent absolutely permanent varieties, or may be explained by gradual modifying changes.

Professor Ansted, summing up the evidence on this head as applied to one division of stratified rocks, writes as follows:

'Palæontologists have endeavoured to separate the Lias into a number of subdivisions, by the Ammonites, groups of species of those shells being characteristic of different zones. The evidence on this point rests on the assumption of specific differences being indicated by permanent modifications of the structure of the shell. But it is quite possible that these may mean nothing more than would be due to some change in the conditions of existence. Except between the Marlstone and the Upper Lias there is really no palæontological break, in the proper sense of the words; alterations of form and size consequent on the occurrence of circumstances more or less favourable, migration of species, and other well-known causes, sufficiently account for many of those modifications of the form of the shell that have been taken as specific marks. This view is strengthened by the fact that other shells and other organisms generally show no proof of a break of any importance except at the point already alluded to.'

But, irrespectively of another deficiency in the geological record, which will be noticed presently, the physical breaks in the stratification make it next to impossible to fairly trace the order of succession of organisms by the evidence afforded by their fossil remains. Thus there are nine great breaks in the Palæozoic series, four in the Secondary, and one in the Tertiary, besides those between Palæozoic and Secondary and Secondary and Tertiary respectively. Thus, in England there are sixteen important breaks in the succession of strata, together with a number of less important interruptions. But although these breaks exist, we find pervading the works of many geologists a belief, resulting from the evidence presented to their minds, sometimes avowed, sometimes unconsciously implied, that the succession of species bears some definite relation to the succession of strata. Thus, Professor Ramsay says, that 'in cases of superposition of fossiliferous strata, in proportion as the species are more or less continuous, that is to say, as the break in the succession of life is partial or complete, so was the time that elapsed between the close of the lower and the commencement of the upper strata a shorter or a longer interval. The break in life may be indicated not only by a difference in species, but yet more importantly by

the absence of older and appearance of newer allied or unallied genera.'

Indications of the connection between cosmical studies and geological researches are dawning on us: there is, for instance, some reason to believe that we can trace many geological phenomena to our varying path of rotation round the sun; thus, more than thirty years ago Sir J. Herschel proposed an explanation of the changes of climate on the earth's surface as evidenced by geological phenomena, founded on the changes of eccentricity in the earth's orbit.

He said he had entered on the subject 'impressed with the magnificence of that view of geological revolutions which regards them rather as regular and necessary effects of great and general causes, than as resulting from a series of convulsions and catastrophes regulated by no laws and reducible to no fixed principles.'

As the mean distance of the earth from the sun is nearly invariable, it would seem at first sight that the mean annual supply of light and heat received by the earth would also be invariable; but according to his calculations it is inversely proportional to the minor axis of the orbit: this would give less heat when the eccentricity of the earth's orbit is approaching towards or at its minimum. Mr. Croll has recently shown reason to believe that the climate, at all events in the circumpolar and temperate zones of the earth, would depend, in some degree, on whether the winter of a given region occurred when the earth at its period of greatest eccentricity was in aphelion or perihelion—if the former, the annual average of temperature would be lower, if the latter, it would be higher than when the eccentricity of the earth's orbit were less, or approached more nearly to a circle. He calculates the difference in the amount of heat at the period of maximum eccentricity of the earth's orbit to be at nineteen to twenty-six, according as the winter would take place when the earth was in aphelion or in perihelion. His reason may be briefly stated thus: assuming the mean annual heat to be the same, whatever the eccentricity of orbit, yet if the extremes of heat and cold in summer and winter be greater, a colder climate will prevail, for there will be more snow and ice accumulated in

the cold winter than the hot summer can melt, a result aided by the shelter from the sun's rays produced by the vapour suspended in consequence of aqueous evaporation; hence we should get glacial periods, when the orbit of the earth is at its greatest eccentricity, at those parts of the earth's surface where it is winter when the earth is in aphelion; carboniferous or hot periods where it is winter in perihelion; and normal or temperate periods when the eccentricity of orbit is at a minimum; all these would gradually slide into each other, and would produce at long distant periods alternations of cold and heat, several of which we actually observe in geological records.

If this theory be borne out, we should approximate to a test of the time which has elapsed between different geological epochs. Mr. Croll's computation of this would make it certainly not less than 100,000 years since the last glacial epoch, a time not very long in geological chronology—probably it is much more.

When we compare with the old theories of the earth, by which the apparent changes on its surface were accounted for by convulsions and cataclysms, the modern view inaugurated by Lyell, your former President, and now, if not wholly, at all events to a great extent adopted, it seems strange that the referring past changes to similar causes to those which are now in operation should have remained uninvestigated until the present century; but with this, as with other branches of knowledge, the most simple is frequently the latest view which occurs to the mind. It is much more easy to invent a *Deus ex machinâ* than to trace out the influence of slow continuous change; the love of the marvellous is so much more attractive than the patient investigation of truth, that we find it to have prevailed almost universally in the early stages of science.

In astronomy we had crystal spheres, cycles, and epicycles; in chemistry the philosopher's stone, the elixir vitæ, the archæus or stomach demon, and phlogiston; in electricity the notion that amber possessed a soul, and that a mysterious fluid could knock down a steeple. In geology a deluge or a volcano was supplied. In palæontology a new race was created whenever

theory required it: how such new races began, the theorist did not stop to enquire.

A curious speculator might say to a palæontologist of even recent date, in the words of Lucretius,—

> 'Nam neque de cœlo cecidisse animalia possunt
> Nec terrestria de salsis exisse lacunis.
> . . . . .
> E nihilo si crescere possent,
> (Tum) fierent juvenes subito ex infantibus parvis,
> E terrâque exorta repente arbusta salirent;
> Quorum nil fieri manifestum est, omnia quando
> Paulatim crescunt, ut par est, semine certo,
> Crescentesque genus servant'

which may be thus freely paraphrased: 'You have abandoned the belief in one primæval creation at one point of time, you cannot assert that an elephant existed when the first saurians roamed over earth and water. Without, then, in any way limiting Almighty power, if an elephant were created without progenitors, the first elephant must, in some way or other, have physically arrived on this earth. Whence did he come? did he fall from the sky (*i.e.* from interplanetary space)? did he rise moulded out of a mass of amorphous earth or rock? did he appear out of the cleft of a tree? If he had no antecedent progenitors, some such beginning must be assigned to him.' I know of no scientific writer who has, since the discoveries of geology have become familiar, ventured to present in intelligible terms any definite notion of how such an event could have occurred: those who do not adopt some view of continuity are content to say God willed it; but would it not be more reverent and more philosophical to enquire by observation and experiment, and to reason from induction and analogy, as to the probabilities of such frequent miraculous interventions?

I know I am touching on delicate ground, and that a long time may elapse before that calm enquiry after truth which it is the object of associations like this to promote can be fully attained; but I trust that the members of this body are sufficiently free from prejudice, whatever their opinions may be, to admit an enquiry into the general question whether what we

term species are and have been rigidly limited, and have at numerous periods been created complete and unchangeable, or whether, in some mode or other, they have not gradually and indefinitely varied, and whether the changes due to the influence of surrounding circumstances, to efforts to accommodate themselves to surrounding changes, to what is called natural selection, or to the necessity of yielding to superior force in the struggle for existence, as maintained by our illustrious countryman Darwin, and other causes, have not so modified organisms as to enable them to exist under changed conditions. I am not going to put forward any theory of my own, I am not going to argue in support of any special theory, but having endeavoured to show how, as science advances, the continuity of natural phenomena becomes more apparent, it would be cowardice not to present some of the main arguments for and against continuity as applied to the history of organic beings.

As we detect no such phenomenon as the creation or spontaneous generation of vegetables and animals which are large enough for the eye to see without instrumental assistance, as we have long ceased to expect to find a Plesiosaurus spontaneously generated in our fish-pond, or a Pterodactyle in our pheasant-cover, the field of this class of research has become identified with the field of the microscope, and at each new phase the investigation has passed from a larger to a smaller class of organisms. The question whether among the smallest and apparently the most elementary forms of organic life the phenomenon of spontaneous generation obtains, has recently formed the subject of careful experiment and animated discussion in France. If it could be found that organisms of a complex character were generated without progenitors out of amorphous matter, it might reasonably be argued that a similar mode of creation might obtain in regard to larger organisms. Although we see no such phenomenon as the formation of an animal such as an elephant, or a tree such as an oak, excepting from a parent which resembles it, yet if the microscope revealed to us organisms, smaller but equally complex, so formed without having been reproduced, it would render it not improbable that such might have been the case

with larger organic beings. The controversy between M. Pasteur and M. Pouchet has led to a very close investigation of this subject, and the general opinion is, that when such precautions are taken as exclude from the substance submitted to experiment all possibility of germs from the atmosphere being introduced, as by passing the air which is to support the life of the animalculæ through tubes heated to redness and other precautions, no formation of organisms takes place. Some experiments of Mr. Child's, communicated to the Royal Society, again throw doubt on the negative results obtained by M. Pasteur; so that the question may be not finally determined, but the balance of experiment and opinion is against spontaneous generation.

One argument presented by M. Pasteur is well worthy of remark, viz. that in proportion as our means of scrutiny become more searching, heterogeny, or the development of organisms without generation from parents of similar organism, has been gradually driven from higher to lower forms of life, so that if some apparent exceptions still exist they are of the lowest and simplest forms, and these exceptions may probably be removed, as M. Pasteur considers he has removed them, by a more searching investigation.

If it be otherwise, if heterogeny obtains at all, few will not now admit that at present the result of the most careful experiments shows it to be confined to the more simple organic structures, and that all the progressive and more highly developed forms are, as far as the most enlarged experience shows, generated by reproduction.

[It has seemed to some an inconsistency that an advocate of continuity or gradual hereditary change in organisms should not be in favour of heterogeny, since, if highly complex organisms be formed by gradual evolution from more simple structures, we should ultimately reach a point at which organised forms should be developed from inorganic matter.

This may be so, could we go far enough back in the history of genesis; but what I look on with some degree of scepticism is the development of complex though minute organisms which appear, so to speak, full-blown at their inception, and perfectly capable of reproduction, though not themselves

reproductions. That there was a time, and possibly may be now, when inorganic matter, by infinitesimally slow changes, became or becomes organised, I am far from denying. All I hesitate to accept is heterogeny of self-moving reproductive organisms, which seem less complex than larger vitalised beings only in all probability because our microscopes are not powerful enough to detect their structures.]

The great difficulty which is met with at the threshold of enquiry into the origin of species is the definition of species; in fact, species can hardly be defined without begging the question in dispute.

Thus, if species be said to be a perseverance of type incapable of blending itself with other types, or, which comes nearly to the same thing, incapable of producing by union with other types offspring of an intermediate character which can again reproduce, we arrive at this result, that whenever the advocate of continuity shows a blending of what had been hitherto deemed separate species, the answer is, they were considered separate species by mistake; they do not now come under the definition of species, because they interbreed.

The line of demarcation is thus *ex hypothesi* removed a step farther, so that unless the advocate of continuity can, on his side, prove the whole question in dispute, by showing that all can directly or by intermediate varieties reproduce, he is defeated by the definition itself of species.

On the other hand, if this, or something in fact amounting to it, be not the definition of species—if it be admitted that distinct species can, under certain favourable conditions, produce intermediate offspring capable of reproduction, then continuity in some mode or other is admitted.

The question, then, takes this form: Are there species or are there not? Is the word to be used as signifying a real, natural distinction, or as a mere convenient designation applied to subdivisions, having a permanence which will probably outlive man's discussions on the subject, but not an absolute fixity? The same question, in a wider sense, and taking into consideration a much longer time, would be applicable to genera and families.

Actual experiment has done little to elucidate the ques-

tion, nor, unless we can suppose the experiments continued through countless generations, is it likely to contribute much to its solution. We must therefore have recourse to the enlarged experience or induction from the facts of geology, palæontology, and physiology, aided by analogy from the modes of action which nature evidences in other departments.

The doctrine of gradual succession is hardly yet formularised ; and though there are some high authorities for certain modifications of such view, the preponderance of authority would necessarily be on the other side. Geology and palæontology are recent sciences, and we cannot tell what the earlier authors would have thought or written had the more recently discovered facts been presented to their view. Authority, therefore, does not much help us on this question.

Geological discoveries seemed, in the early period of the science, to show complete extinction of certain species and the appearance of new ones, great gaps existing between the characteristics of the extinct and the new species. As science advanced, these were more or less filled up, and the difficulty in the first instance of admitting unlimited modification of species would seem to have arisen from the comparison of the extreme ends of the scale where the intermediate links or some of them were wanting.

To suppose a Zoophyte the progenitor of a Mammal, or to suppose at some particular period of time a highly developed animal to have come out of nothing, or suddenly grown out of inorganic matter, would appear at first sight equally extravagant hypotheses. As an effort of Almighty creative power, neither of these alternatives presents more difficulty than the other ; but as we have no means of ascertaining how creative power worked, but by an examination and study of the works themselves, we are not likely to get either view proved to ocular demonstration. A single phase in the progress of natural transmutation would probably require a term far transcending all that embraced by historical records ; and, on the other hand, it might be said, sudden creations, though taking place frequently, if viewed with reference to the immensity of time involved in geological periods, may be so rare with reference to our experience, and so difficult of clear

authentication, that the non-observation of such instances cannot be regarded as absolute disproof of their possible occurrence.

The more the gaps between species are filled up by the discovery of intermediate varieties the stronger becomes the argument for transmutation and the weaker that for successive creations, because the former view then becomes more and more consistent with experience, the latter more discordant from it. As undoubted cases of variation, more or less permanent from given characteristics, are produced by the effects of climate, food, domestication, &c., the more species are increased by intercalation, the more the distinctions slide down towards those which are within the limits of such observed deviations; while, on the other hand, to suppose the more and more frequent recurrence of fresh creations out of amorphous matter is a multiplication of miracles or special interventions not in accordance with what we see of the uniform and gradual progress of nature, either in the organic or inorganic world. If we were entitled to conclude that the progress of discovery would continue in the same course, and that species would become indefinitely multiplied, the distinctions would become infinitely minute, and all lines of demarcation would cease, the polygon would become a circle, the succession of points a line. Certain it is that the more we observe the more we increase the subdivision of species, and consequently the number of the supposed creations; so that new creations become innumerable; and yet of these we have no one well-authenticated instance, and in no other observed operation of nature have we seen this want of continuity, these frequent *per saltum* deviations from uniformity, each of which is a miracle.

The difficulty of producing intermediate offspring from what are termed distinct species and the infecundity in many instances of hybrids are used as strong arguments against continuity of succession; on the other hand, it may be said long-continued variation through countless generations has given rise to such differences of physical character that reproduction is difficult in some cases, and in others impossible.

Suppose, for instance, M to represent a parent race whose

offspring by successive changes through eons of time have divaricated, and running opposite ways which we may denote by the letters of the alphabet, have produced on the one hand a species A, and on the other a species Z: the changes here have been so great that we should never expect directly to reproduce an intermediate between A and Z. A and B on the one hand, and Y and Z on the other, might reproduce; but to regain the original type M, we must not only retrocede through all the intermediates, but must have similar circumstances recalled in an inverse order at each phase of retrogression, conditions which it is obviously impossible to fulfil. But though among the higher forms of organic structure we cannot retrace the effects of time and reproduce intermediate types, yet among some of the lower forms we find it difficult to assign any line of specific demarcation; thus, as a result of the very elaborate and careful investigations of Dr. Carpenter on Foraminifera, he states: 'It has been shown that a very wide range of variation exists among Orbitolites, not merely as regards external form, but also as to plan of development; and not merely as to the shape and aspect of the entire organism, but also with respect to the size and configuration of its component parts. It would have been easy, by selecting only the most divergent types from amongst the whole series of specimens which I have examined, to prefer an apparently substantial claim on behalf of these to be accounted as so many distinct species. But after having classified the specimens which could be arranged around these types, a large proportion would yet have remained, either presenting characters intermediate between those of two or more of them, or actually combining those characters in different parts of their fabric; thus showing that no lines of demarcation can be drawn across any part of the series that shall definitely separate it into any number of groups, each characterised by features entirely peculiar to itself.'

At the conclusion of his enquiry he states—

I. The range of variation is so great among Foraminifera as to include not merely the differential characters which systematists proceeding upon the ordinary methods have accounted specific, but also those upon which the greater part of the

genera of this group have been founded, and even in some instances those of its orders.

II. The ordinary notion of species as assemblages of individuals marked out from each other by definite characters that have been genetically transmitted from original proto-types similarly distinguished, is quite inapplicable to this group; since even if the limits of such assemblages were extended so as to include what elsewhere would be accounted genera, they would still be found so intimately connected by gradational links that definite lines could not be drawn between them.

III. The only natural classification of the vast aggregate of diversified forms which this group contains will be one which ranges them according to their direction and degree of divergence from a small number of principal family types; and any subordinate grouping of genera and species which may be adopted for the convenience of description and nomenclature must be regarded merely as assemblages of forms characterised by the nature and degree of the modifications of the original type, which they may have respectively acquired in the course of genetic descent from a common ancestry.

IV. Even in regard to these family types it may fairly be questioned whether analogical evidence does not rather favour the idea of their derivation from a common original than that of their primitive distinctness.

Mr. H. Bates, when investigating 'The Lepidoptera of the Amazon Valley,' may almost be said to have witnessed the origin of some species of butterflies, so close have been his observations on the habits of these animals that have led to their variation and segregation, so closely do the results follow his observations, and so great is the difficulty of otherwise accounting for any of the observed facts.

In the numerous localities of the Amazon region certain gregarious species of butterfly (*Heliconidea*) swarm in incredible numbers, almost outnumbering all the other butterflies in the neighbourhood; the species in the different localities being different, though often to be distinguished by a very slight shade.

In these swarms are to be found, in small numbers, other species of butterflies belonging to as many as ten different genera, and even some moths; and these intruders, though they structurally differ *in toto* from the swarms they mingle with, and from one another, mimic the *Heliconidea* so closely in colours, habits, mode of flight, &c., that it is almost impossible to distinguish the intruders from those they mingle with. The obvious benefit of this mimicry is safety, the intruders hence escaping detection by predatory animals.

Mr. Bates has extended his observations to the habits of life, food, variations, and geographical range of the species concerned in these mimetic phenomena, and finds in every case corroborative evidence of every variety and species being derivative, the species being modified from place to place to suit the peculiar form of *Heliconidea* stationed there.

Mr. Wallace has done similar service to the derivative theory by his observations and writings on the butterflies and birds of the Malay Archipelago, adducing instances of mimetic resemblances strictly analogous to the above; and adding in further illustration a beautiful series of instances where the form of the wing of the same butterfly is so modified in various islets as to produce changes in their mode of flight that tend to the conservation of the variety by aiding its escape when chased by birds or predacious insects.

He has also adduced a multitude of examples of geographical and representative races, species, and varieties, forming so graduated a series as to render it obvious that they have had a common origin.

The effect of food in the formation and segregation of races and of certain groups of insects has been admirably demonstrated by Mr. B. D. Walsh, of North America.

Mr. McDonnell has been led to the discovery of a new organ in electric fishes from the application of the theory of descent, and Dr. Fritz Müller has published numerous observations showing that organs of very different structure may, through the operation of natural selection, acquire very similar and even identical functions. Sir John Lubbock's diving hymenopterous insect affords a remarkable illustration of

analogous phenomena; it dives by the aid of its wings, and is the only insect of the vast order it belongs to that is at all aquatic.

The discovery of the Eozoon is of the highest importance in reference to the derivative hypothesis, occurring as it does in strata that were formed at a period inconceivably antecedent to the presupposed introduction of life upon the globe, and displacing the argument derived from the supposition that at the dawn of life a multitude of beings of high organisation were simultaneously developed (in the Silurian and Cambrian strata).

Professor A. De Candolle, one of the most distinguished Continental botanists, has, to some extent, abandoned the tenets held in his 'Géographie Botanique,' and favours the derivative hypothesis in his paper on the variation of oaks; following up a paper by Dr. Hooker, on the oaks of Palestine, showing that some sixteen of them are derivative, he avows his belief that two-thirds of the 300 species of this genus, which he himself describes, are provisional only.

Dr. Hooker, who had only partially accepted the derivative hypothesis propounded before the publication of 'The Origin of Species through Natural Selection,' at the same time declining the doctrine of special creation, has since then cordially adopted the former, and illustrated its principles by applying them to the solution of various botanical questions: first, in reference to the flora of Australia, the anomalies of which he appears to explain satisfactorily by the application of these principles; and, latterly, in reference to the Arctic flora.

In the case of the Arctic flora, he believes that originally Scandinavian types were spread over the high northern latitudes; that these were driven southwards during the glacial period, when many of them changed their forms in the struggle that ensued with the displaced temperate plants; that on the returning warmth, the Scandinavian plants, whether changed or not, were driven again northwards and up to the mountains of the temperate latitudes, followed in both cases by series of pre-existing plants of the temperate Alps. The result is the present mixed Arctic flora, consisting of a basis of more or less changed and unchanged Scandinavian plants, associated

in each longitude with representatives of the mountain flora of the more temperate regions to the south of them.

The publication of a previously totally unknown flora, that of the Alps of tropical Africa, by Dr. Hooker, has afforded a multitude of facts that have been applied in confirmation of the derivative hypothesis. This flora is found to have relationships with those of temperate Europe and North Africa, of the Cape of Good Hope, and of the mountains of tropical Madagascar and Abyssinia, such as can be accounted for on no other hypothesis but that there has been ancient climatal connection and some coincident or subsequent slight changes of specific character.

The doctrine of Cuvier, every day more and more borne out by observation, that each organ bears a definite relation to the whole of the individual, seems to support the view of indefinite variation. If an animal seeks its food or safety by climbing trees, its claws will become more prehensile, the muscles which act upon those claws must become more developed, the body will become agile by the very exercise which is necessary to it, and each portion of the frame will mould itself to the wants of the animal by the effect on it of the habits of the animal.

Another series of facts which present an argument in favour of gradual succession, are the phases of resemblance to inferior orders which the embryo passes through in its development, and the relations shown in what is termed the metamorphosis of plants; facts difficult to account for on the theory of frequent separate creations, but almost inevitable on that of gradual succession. So also the existence of rudimentary and effete organs, which must either be referred to a *lusus naturæ* or to some mode of continuous succession.

The doctrine of typical nuclei seems only a mode of evading the difficulty; experience does not give us the types of theory; and, after all, what are these types? It must be admitted there are none such in reality; how are we led to the theory of them? simply by a process of abstraction from classified existences. Having grouped from natural similitudes certain forms into a class, we select attributes common to each member of the class, and call the assemblage of such attri-

butes a type of the class. This process gives us an abstract idea, and we then transfer this idea to the Creator, and make him start with that which our own imperfect generalisation has derived. It seems to me that the doctrine of types is, in fact, a concession to the theory of continuity or indefinite variability; for the admission that large groups have common characters shows, necessarily, a blending of forms within the scope of the group, which supports the view of each member being derived from some other member of it: if this be so can it be fairly asserted that even the assigned limits of such groups have a definite line of demarcation?

[The constant use of different terms has frequently led to a settled conviction that the things signified are as broadly distinct as the terms used. Take, for instance, the terms 'instinct' and 'reason;' for centuries these have been regarded as distinct mental attributes, and it is only in recent years that the merging of the one into the other has been discussed even as a possibility.

Is what is called instinct other than a process of reasoning carried on in a limited sphere, and very perfect within that sphere, because from long habit and hereditary transmission it has become stereotyped in the race?

The bee has learned gradually by using mere mechanical means to save space by forcing the cylindrical cells of its honeycomb against each other, until by pressing out the intervening matter they become hexagons, and not by a mathematical instinct, as it used to be called. By similar transmitted habits he knows the honey-bearing flowers by their colour or their scent. The mind of the higher animals—the dog, for instance—is more diffuse, but attains less perfection in any limited sphere; his wants and habits being more various, his brain has more varied power; but when he buries a bone or points at a partridge I see no reason for supposing that he feels compelled by some power extraneous to himself to do these acts, but probably feels as much acting from the impulse of his own mind as an Esquimaux making a 'cache' or lying in wait for a seal. Thoughts which may seem intuitive in the individual are probably the result of experience accumulated and transmitted in the race, and gradually modifying the structure of the brain

in the course of transmission. This has long been my opinion, and, as I understand, it is the view advocated by Mr. Herbert Spencer in different parts of his valuable works.]

The condition of the earth's surface, or at least of large portions of it, has for long periods remained substantially the same; this would involve a greater degree of fixity in the organisms which have existed during such periods of little change than in those which have come into being during periods of more rapid transition; for, though rejecting catastrophes as the *modus agendi* of nature, I am far from saying that the march of physical changes has been always perfectly uniform.

There have been, doubtless, what may be termed secular seasons, and there have been local changes of varying degrees of extent and permanence; from such causes the characteristics of organised beings would be more concentrated in certain directions than in others, the fixity of character being in the ratio of the fixity of condition. This would throw natural forms into certain groups which would be more prominent than others, like the colours of the rainbow, which present certain predominant tints, though they merge into each other by insensible gradations.

While the evidence seems daily becoming stronger in favour of a derivative hypothesis as applied to the succession of organic beings, we are far removed from anything like a sufficient number of facts to show that, at all events within the existing geological periods capable of being investigated, there has been always a progression from a simpler or more embryonic to a more complex type.

Professor Huxley, though inclined to the derivative hypothesis, shows, in the concluding portion of his address to the Geological Society, 1862, a great number of cases in which, though there is abundant evidence of variation, there is none of progression. There are, however, several groups of Vertebrata in which the endoskeleton of the older presents a less ossified condition than that of the younger genera. He cites the Devonian Ganoids, the Mesozoic Lepidosteidæ, the Palæozoic Sharks, and the more ancient Crocodilia and Lacertilia, and particularly the Pycnodonts and Labyrintho-

donts, as instances of this when compared with their more recent representatives.

The records of life on the globe may have been destroyed by the fusion of the rocks, which would otherwise have preserved them, or by crystallisation after hydrothermal action. The earlier forms may have existed at a period when this planet was in course of formation, or being segregated or detached from other worlds or systems. We have hardly evidence enough to speculate on the subject, but by time and patience we may acquire it.

Were all the forms which have existed embalmed in rock the question would be solved; but what a small proportion of extinct forms is so preserved, and must be, if we consider the circumstances necessary to fossilise organic remains! On the dry land, unwashed by rivers and seas, when an animal or plant dies, it undergoes chemical decomposition, which changes its form; it is consumed by insects, its skeleton is oxidised and crumbles into dust. Of the myriads of animals and vegetables which annually perish we find hardly an instance of a relic so preserved as to be likely to become a permanent fossil. So again in the deeper parts of the ocean, or of the larger lakes, the few fish there are perish and their remains sink to the bottom, and are there frequently consumed by other marine or lacustrine organisms or are chemically decomposed. As a general rule, it is only when the remains are silted up by marine, fluviatile or lacustrine sediments that the remains are preserved. Geology, therefore, might be expected to keep for us mainly those organic remains which inhabited deltas or the margins of seas, lakes, or rivers; here and there an exception may occur, but the mass of preserved relics would be those of creatures so situated; and so we find it, the bulk of fossil remains consists of fish and amphibia; shell-fish form the major part of the geological museum, limestone and chalk rocks frequently consisting of little else than a congeries of fossil shells; plants of reed or rush-like character, fish which are capable of inhabiting shallow waters, and saurian animals, form another large portion of geological remains.

Compare the shell-fish and amphibia of existing organisms

with the other forms, and what a small proportion they supply; compare the shell-fish and amphibia of Palæontology with the other forms, and what an overwhelming majority they yield!

There is nothing, as Professor Huxley has remarked, like an extinct order of birds or mammals, only a few isolated instances. It may be said the ancient world possessed a larger proportion of fish and amphibia, and was more suited to their existence. I see no reason for believing this, at least to anything like the extent contended for; the fauna and flora now in course of being preserved for future ages would give the same idea to our successors.

Crowded as Europe is with cattle, birds, insects, &c., how few are geologically preserved! while the muddy or sandy margins of the ocean, the estuaries, and deltas are yearly accumulating numerous crustacea and mollusca, with some fishes and reptiles, for the study of future palæontologists.

If this position be right, then, notwithstanding the immense number of preserved fossils, there must have lived an immeasurably larger number of organic beings which are not preserved, so that the chance of filling up the missing links, except in occasional instances, is very slight. Yet, where circumstances have remained suitable for their preservation, many closely connected species are preserved—in other words, while the intermediate types in certain cases are lost, in others they exist. The opponents of continuity lay all stress on the lost and none on the existing links.

But there is another difficulty in the way of tracing a given organism to its parent forms, which, from our conventional mode of deducing genealogies, is never looked upon in its proper light.

Where are we to look for the remote ancestor of a given form? Each of us, supposing none of our progenitors to have intermarried with relatives, would have had at or about the period of the Norman Conquest upwards of a hundred million direct ancestors of that generation, and if we add the intermediate ancestors, double that number. As each individual has a male and female parent, we have only to

multiply by two for each thirty years, the average duration of a generation, and it will give the above result.

Let anyone assume that one of his ancestors at the time of the Norman Conquest was a Moor, another a Celt, and a third a Laplander, and that the remains of these three were preserved, while those of all the others were lost, he would never recognise either of them as his ancestor; he would only have the one-hundred millionth of the blood of each of them, and, as far as they were concerned, there would be no perceptible sign of identity of race.

But the problem is more complex than that which I have stated. At the time of the Conquest there were hardly a hundred million people in Europe: it follows that a great number of the ancestors of the *propositus* must have intermarried with relations, and then the pedigree, going back to the time of the Conquest, instead of being represented by diverging lines, would form a network so tangled that no skill could unravel it. The law of probabilities would indicate that any two people in the same country, taken at hazard, would not have many generations to go back before they would find a common ancestor, who, probably, could they have seen him or her in the life, had no traceable resemblance to either of them. Thus, two animals of a very different form, and of what would be termed very different species, might have a common geological ancestor, and yet the skill of no comparative anatomist could trace the descent.

From the long-continued conventional habit of tracing pedigrees through the male ancestor, we forget, in talking of progenitors, that each individual has a mother as well as a father, and there is no reason to suppose that he has in him less of the blood of the one than of the other.

The recent discoveries in palæontology show us that Man existed on this planet at an epoch far anterior to that commonly assigned to him. The instruments connected with human remains, and indisputably the work of human hands, show that to these remote periods the term civilisation could hardly be applied—chipped flints of the rudest construction, probably, in the earlier cases, fabricated by holding an amorphous flint in the hand and chipping off portions of it by

striking it against a larger stone or rock; then, as time suggested improvements, it would be more carefully shaped, and another stone used as a tool; then (at what interval we can hardly guess) it would be ground, then roughly polished, and so on—subsequently bronze weapons, and nearly the last before we come to historical periods, iron. Such an apparently simple invention as a wheel must, in all probability, have been far subsequent to the rude hunting tools or weapons of war to which I have alluded.

A little step-by-step reasoning will convince the unprejudiced that what we call civilisation must have been a gradual process; can it be supposed that the inhabitants of Central America or of Egypt suddenly and what is called instinctively built their cities, carved and ornamented their monuments? If not, if they must have learned to construct such erections, did it not take time to acquire such learning, to invent tools as occasion required, contrivances to raise weights, rules or laws by which men acted in concert to effect the design? Did not all this require time? and if, as the evidence of historical times shows, invention marches with a geometrical progression, then, viewing its progress inversely, how slow must have been the earlier steps! If even now habit, and prejudice resulting therefrom, vested interests, &c., retard for some time the general application of a new invention, or the adoption of a new social change, what must have been the degree of retardation among the comparatively uneducated beings which then existed?

I have of course been able to indicate only a few of the broad arguments on this most interesting subject; for detailed data on which my reasoning is founded, the works of Lamarck, Darwin, Hooker, Huxley, Carpenter, Lyell, and others must be examined. If I lean to the view that the successive changes in organic beings do not take place by sudden leaps, it is, I believe, from no want of an impartial feeling; but if the facts are stronger in favour of one theory than another, it would be an affectation of impartiality to make the balance appear equipoised.

The prejudices of education and associations with the past are against this, as against all new views; and while on the

one hand a theory is not to be accepted because it is new and *primâ facie* plausible, still to this assembly I need not say that its running counter to existing opinions is not necessarily a reason for its rejection; the *onus probandi* should rest on those who advance a new view, but the degree of proof must differ with the nature of the subject. The fair question is, Does the newly-proposed view remove more difficulties, require fewer assumptions, and present more consistency with observed facts than that which it seeks to supersede? If so, the philosopher will adopt it, and the world will follow the philosopher—after many days.

It must be borne in mind that, even if we are satisfied from a persevering and impartial enquiry that organic forms have varied indefinitely in time, the *causa causans* of these changes is not explained by our researches; if it be admitted that we find no evidence of amorphous matter suddenly changed into complex structure, still why matter should be endowed with the plasticity by which it slowly acquires modified structure is unexplained. If we assume that continuous effort, natural selection, or the struggle for existence, coupled with the tendency of like to reproduce like, gives rise to various organic changes, still our researches are at present uninstructive as to why like should produce like, why acquired characteristics in the parent should be reproduced in the offspring. Reproduction itself is still an enigma, and this great question may involve deeper thoughts than it would be suitable to enter upon now.

Perhaps *primâ facie* the most striking argument in favour of continuity which could be presented to a doubting mind would be the difficulty it would feel in representing to itself any *per saltum* act of nature. Who would not be astonished at beholding an oak tree spring up in a day, and not from seed or shoot? We are forced by experience, though often unconsciously, to believe in continuity as to all effects now taking place; if any one of them be anomalous we endeavour, by tracing its history and concomitant circumstances, to find its cause, *i.e.* to relate it to antecedent phenomena; are we then to reject similar enquiries as to the past? Is it laudable to seek an explanation of present changes by observation,

experiment, and analogy, and yet reprehensible to apply the same mode of investigation to the past history of the earth and of the organic remains embalmed in it?

If we disbelieve in sudden creations of matter or force, in the sudden formations of complex organisms now, if we now assign to the heat of the sun an action enabling vegetables to live by assimilating gases, carbon, and amorphous earths into growing structures, why should such effects not have taken place in earlier periods of the world's history, when the sun shone as now, and when the same materials existed for his rays to fall upon?

If we are satisfied that continuity is a law of nature, the true expression of the action of Almighty Power, then, though we may humbly confess our inability to explain why matter is impressed with this tendency to gradual structural formation, we should cease to look for special interventions of creative power in changes which are difficult to understand, because, being removed from us in time, their concomitants are lost; we should endeavour from the relics to evoke their history, and when we find a gap not try to bridge it over with a miracle.

If it be true that continuity pervades all physical phenomena, the doctrine applied by Cuvier to the relations of the different parts of an animal to each other might be capable of great extension. All the phenomena of inorganic and organised matter might be expected to be so inter-related that the study of an isolated phenomenon would lead to a knowledge of numerous other phenomena with which it is connected. As the antiquary deduces from a monolith the tools, the arts, the habits, and epoch of those by whom it is wrought, so the student of science may deduce from a spark of electricity or ray of light the source whence it is generated; and by similar processes of reasoning other phenomena hitherto unknown may be deduced from their probable relation with the known. But, as with heat, light, magnetism, and electricity, though we may study the phenomena to which these names have been given, and their mutual relations, we know nothing of their ultimate cause; so, whether we adopt the view of natural selection, of effort, of plasticity, &c., we know not

why organisms should have this *nisus formativus*, or why the acquired habit or exceptional quality of the individual should reappear in the offspring.

Philosophy ought to have no likes or dislikes—truth is her only aim; but if a glow of admiration be permitted to a physical enquirer, to my mind a far more exquisite sense of the beautiful is conveyed by the orderly development, by the necessary inter-relation and inter-action of each element of the cosmos, and by the conviction that a bullet falling to the ground changes the dynamical conditions of the universe, than can be conveyed by mysteries, by convulsions, or by cataclysms.

The sense of understanding is to the educated more gratifying than the love of the marvellous, though the latter need never be wanting to the nature-seeker.

But the doctrine of continuity is not solely applicable to physical enquiries.

The same modes of thought which lead us to see continuity in the field of the microscope as in the universe, in infinity downwards as in infinity upwards, will lead us to see it in the history of our own race; the revolutionary ideas of the so-called natural rights of man, and *à priori* reasoning from what are termed first principles, are far more unsound and give us far less ground for improvement of the race than the study of the gradual progressive changes arising from changed circumstances, changed wants, changed habits. Our language, our social institutions, our laws, the constitution of which we are proud, are the growth of time, the product of slow adaptations, resulting from continuous struggles. Happily in this country practical experience has taught us to improve rather than to remodel; we follow the law of nature and avoid cataclysms.

[My own impression is that the philosophy of the future, not merely as applied to physical forces and the science of organisms, but to the history of the human race, its habits, laws, languages, and possibly thoughts themselves, will be mainly based on the doctrine of continuity, and that instead of enquiries as to *why* a thing is in the sense of ascertaining its ultimate causation, the research will be into the question

how did it become what it is? by what steps of change, by what mode of force did the substance, the phenomenon, the organism, the habit or the event arise?]

The superiority of Man over other animals inhabiting this planet, of civilised over savage man, and of the more civilised over the less civilised, is proportioned to the extent which his thought can grasp of the past and of the future. His memory reaches farther back, his capability of prediction reaches farther forward in proportion as his knowledge increases. He has not only personal memory which brings to his mind at will the events of his individual life—he has history, the memory of the race; he has geology, the history of this planet; he has astronomy, the geology of other worlds. Whence does the conviction to which I have alluded, that each material form bears in itself the records of its past history, arise? Is it not from the belief in continuity? Does not the worn hollow in the rock record the action of the tide, its stratified layers the slow deposition by which it was formed, the organic remains imbedded in it, the beings living at the times these layers were deposited, so that from a fragment of stone we can get the history of a period myriads of years ago? From a fragment of bronze we may get the history of our race at a period antecedent to tradition. As science advances our power of reading such history improves and is extended. Saturn's ring may help us to a knowledge of how our solar system developed itself, for it as surely contains that history as the rock contains the record of its own formation.

By this patient investigation how much have we already learned, which the most civilised of ancient human races ignored! While in ethics, in politics, in poetry, in sculpture, in painting, we have scarcely, if at all, advanced beyond the highest intellects of ancient Greece or Italy, how great are the steps we have made in physical science and its applications!

But how much more may we not expect to know!

We, this evening assembled, Ephemera as we are, have learned by transmitted labour, to weigh, as in a balance, other worlds larger and heavier than our own, to know the length of their days and years, to measure their enormous distance from us and from each other, to detect and accurately ascertain

the influence they have on the movements of our world and on each other, and to discover the substances of which they are composed; may we not fairly hope that similar methods of research to those which have taught us so much may give our race farther information, until problems relating not only to remote worlds, but possibly to organic and sentient beings which may inhabit them—problems which it might now seem wildly visionary to enunciate—may be solved by progressive improvements in the modes of applying observation and experiment, induction and deduction?

ON A NEW VOLTAIC COMBINATION.

Phil. Mag., 1839.*

Mr. Porrett was, I believe, the first who employed a bladder to separate the liquids in the operating cell of the voltaic pile. M. Becquerel, by introducing this into the exciting cells, has shown us how to render constant the primitive intensity of the battery by preventing cross precipitation ; Mr. Daniell has remedied some practical defects in M. Becquerel's arrangement, and his form of battery is undoubtedly the best of any that have been hitherto proposed.

In a letter published in the 'Philosophical Magazine' for February I endeavoured to show, that in addition to the immense benefit derived from constancy of action, which was the object aimed at by these gentlemen, the combination of four elements was capable of producing a much more powerful development of electricity than that of three, as by this means we have nearly the sum of chemical affinities instead of their difference ; I also there suggested that if the principles I had laid down were true, there was every probability of superior combinations being discovered. I have lately been fortunate enough to hit upon a combination which I have no hesitation in pronouncing much more powerful than any previously known. The experiments which led to it are curious, and possess an interest of their own, as they prove a well-known chemical phenomenon to depend upon electricity, and thus tighten the link which binds these two sciences. The effect to which I allude is the dissolution of gold in nitro-muriatic acid ; this metal, as is well known, not being attacked by either of the acids singly. The following experiments leave, I think, no doubt as to the *rationale* of this phenomenon :—

* From the *Comptes Rendus*, Académie des Sciences, Paris.

1. Into the bottom of a wine glass I cemented the bowl of a tobacco-pipe; into this was poured pure nitric acid, while muriatic acid was poured into the wine glass to the same level; in this latter acid two strips of gold-leaf were allowed to remain for an hour, at the end of which time they remained as bright as when first immersed. A gold wire was now made to touch the nitric acid and the extremity of one of the strips of gold-leaf; this was instantly dissolved, while the other strip remained intact.

2. The experiment was inverted, but offered some difficulty, as the gold would not remain an equally long time in the nitric acid, from the effect of the nitrous gas; enough, however, was ascertained to prove that to the gold in this acid contact made little or no difference; while the gold in the muriatic was always dissolved.

3. A platina arc was used for connection instead of gold; the effect was the same.

4. The outside of the pipe was coated with gold-leaf, leaving scarcely any part exposed; a strip was placed in the muriatic acid as before, and when contact was made with the nitric acid this strip was destroyed, while the coating of gold directly across the line of junction was unhurt.

5. The nitric acid was stained with a little tournesol; when contact was made, I could not see that the muriatic acid acquired any of the colour.

6. Nitrate of copper was used instead of nitric acid; the effect was the same, but took place more slowly, and I could detect no precipitation on the negative metal.

7. I now made gold-leaf in muriatic acid the electrodes of a single pair of voltaic metals; the acid was decomposed and the positive electrode was dissolved.

From all this I think we may pronounce the action to be as follows: as soon as the electric current is established, both the acids are decomposed, the hydrogen of the muriatic unites with the oxygen of the nitric, and the chlorine attacks the gold.

In all these cases the currents were examined with a galvanometer, and in all the gold which was dissolved represented the zinc of an ordinary voltaic combination. The greatest deflection was obtained with platina, gold, and two

acids. It now occurred to me that as gold, platina, and two acids gave so powerful an electric current, *à fortiori*, the same arrangement, with the substitution of zinc for gold, must form a combination more energetic than any yet known. I delayed not to submit this to experiment, and was gratified with the most complete success. A single pair, composed of a strip of amalgamated zinc an inch long and a quarter of an inch wide, a cylinder of platina three-quarters of an inch high, with a tobacco-pipe bowl and an egg-cup, readily decomposed water acidulated with sulphuric acid. In this battery the action is constant, and there is no precipitation on either metal. It offers the great advantage of being able to utilise the action of concentrated nitric acid. I tried the same arrangement, substituting for the muriatic acid caustic potass, which was suggested to me by a well-known experiment of M. Becquerel: the action was equally powerful; and I should prefer this arrangement, as there is no necessity for amalgamating the zinc, but for a fatal objection—the nitrate of potass, crystallising in the pores of the earthenware, splits it to pieces; except, therefore, a new description of diaphragm be discovered, which will bear the action of powerful acids, this combination must be abandoned.

I diluted the muriatic acid with twice its volume of water, and the effect was not perceptibly inferior. I then tried sulphuric with four or five times its volume of water; the intensity was a little diminished, but so little that I should prefer this combination to any other, as cheaper, exercising less local action on the zinc, and by no possibility endangering the platina. The nitric acid may be the common acid of commerce, but must be concentrated. If the hydrogen, instead of being absorbed by the oxygen of the nitric acid, is evolved on the surface of the platina, the energy of action is lowered and is no longer constant.

Great advantage will be found in employing a cell divided by a porous diaphragm for a decomposing cell; thus, if oxygen gas be wanted, the positive electrode should be put into dilute sulphuric acid and the negative into concentrated nitric. If chlorine be wanted, the positive into muriatic, the negative into nitric; if hydrogen, both into muriatic, the positive one

being of amalgamated zinc, &c. &c. By this means, and with a small battery of the description I am about to indicate, a traveller may carry in his pocket an electro-chemical laboratory.

I have constructed a small battery, of a circular shape, consisting of seven liqueur-glasses and seven pipe-bowls; the diameter is four inches, the height one inch and a quarter; this pocket battery gives about one cubic inch of mixed gases in two minutes. The form of this combination is in effect that of Mr. Daniell; the connexions, however, never need adjustment but when the worn-out zinc is renewed; and in a battery which M. Becquerel and myself are about to construct I hope to remedy another defect, viz. the necessity of pouring solutions separately into each cell, which is troublesome and injurious, from the inequality of strength which results. I have entirely remedied this in a copper and zinc constant battery; but though the process be simple, it would require more words to describe than a matter of such minor importance is worth.

ON VOLTAIC REACTION, OR THE PHENOMENA USUALLY CALLED POLARISATION.

Philosophical Mag., 1840. Report of Royal Institution Lecture.

MR. GROVE detailed the first experiments of Volta, Erman, Ritter, and Davy, the more recent ones of De la Rive, the explanation of these by Becquerel, and the confirmation of this latter philosopher's opinion by the experiments of Dr. Schœnbein, Mr. Matteucci, and Mr. Grove himself; all which, as well as the experiments of Mr. Grove on the inactivity of amalgamated zinc, which he proved to be due to the same order of causes, have been already given in full in various numbers of the 'Philosophical Magazine.' All the effects which have generally been included under the term polarisation were proved by Mr. Grove to be traceable to one principle, viz. the electrolytic transfer of elements having for

each other a chemical affinity, and the reaction caused by this affinity when the decomposing and transferring power, *i.e.* the initial voltaic current, is arrested. What we are most anxious to call the attention of our readers to are the effects exhibited by Mr. Grove at the conclusion of his lecture. Two batteries, little differing in construction from that described by him in the 'Lond. and Edinb. Phil. Mag.,' were charged some time previously to the lecture, and up to the period of its conclusion remained in perfect inactivity until the circuit was completed. One of these was arranged as a series of five plates, and contained altogether about four square feet of platina foil; with this the mixed gases were liberated from water at the rate of one hundred and ten cubic inches per minute. A sheet of platinum, one inch wide by twelve long, was heated in the open air through its whole extent, and the usual class of effects produced in corresponding proportion. With the other arrangement, consisting of fifty plates of two inches by four, arranged in single series, a voluminous flame of one inch and a quarter long was exhibited by charcoal points, which showed beautifully the magnetic properties of the voltaic arc, as Dr. Faraday held a piece of iron near it, being attracted and repelled by different portions of the iron: bars of different metals were instantly run into globules and dissipated in oxide. It should be borne in mind that all these effects were produced by a battery which did not cover a space of sixteen inches square, and was only four inches high, and which had been charged for some hours.

Mr. Grove adverted to the letter of Prof. Jacobi to Dr. Faraday published in 'Lond. and Edinb. Phil. Mag.,' vol. xv. p. 161, and stated that Mr. Pattison, who navigated the Neva with Prof. Jacobi in October last, had observed that the batteries employed were on Mr. Grove's construction, which the Professor without hesitation admitted.

ON THE INACTION OF AMALGAMATED ZINC IN ACIDULATED WATER.*

Phil. Mag., August 1839.

It is a well-known fact that the common zinc of commerce, when immersed in water acidulated by sulphuric, phosphoric, or muriatic acid, is rapidly dissolved, evolving torrents of hydrogen gas, while zinc with the surface amalgamated remains inactive under similar circumstances, unless touched by another metal placed in the same solution, in which case hydrogen, amounting to the equivalent of the oxygen which unites with the zinc, is evolved from the surface of the associated metal, and the zinc is tranquilly dissolved.

M. De la Rive observed that pure zinc is much less vigorously attacked by diluted acid than commerical zinc, and has thence, after a careful experimental investigation, concluded that the evolution of gas by common zinc arises from the circumstance of its adulteration by other metals; thus an infinity of minute voltaic circles is established, the particles of zinc being oxidated, while those of the more negative metals evolve hydrogen ('Bib. Univ.,' 1830). This explanation does not apply to the inactivity of amalgamated zinc, for, as M. Becquerel asks ('Traité,' vol. v. p. 8), 'Why does not mercury, which by its contact with zinc and acidulated water must also form a voltaic combination, produce a similar effect?'

An accidental circumstance led me to some experiments which I think give a satisfactory answer to this question. The circumstance to which I allude, and which has in all probability been observed by many others, was this: in decomposing by a voltaic battery water acidulated with sulphuric acid, there happened to be a few globules of mercury at the bottom of the operating cell or glass containing the electrodes of platinum. I remarked that whenever the negative electrode touched the mercury it became amalgamated; at first I attri-

* Read before the Royal Academy of Sciences of Paris, on June 24, 1839.

buted this effect to the reduction of a film of oxide of mercury by the nascent hydrogen, but on touching the negative electrode thus amalgamated with the positive one, the latter also became frequently amalgamated. After several experiments I found that mercury which had acted in acidulated water as negative electrode of a voltaic battery possessed the property of amalgamating platinum and iron, and that strips of platinum, iron, and even steel, which had served as negative electrodes, were instantly and perfectly amalgamated by immersion in pure mercury.

Having cleansed from acid particles several portions of mercury which had been used as negative electrodes, I found that they invariably gave an alkaline reaction, and it now became evident that the increased power of amalgamation proceeded from the mercury being alloyed with an alkaline metal. Remembering the highly electro-positive state of mercury which contains the slightest traces of an alkaline metal, a property first noticed by Sir Humphry Davy, it occurred to me that the inaction of amalgamated zinc was the effect of polarisation,* but of one which differed from ordinary cases of polarisation, in that the cations of the electrolyte instead of being precipitated on the surface of the negative metal, combine with it and render it so completely positive that the current is nullified, and not merely reduced in intensity as in other cases. To verify this idea I made the following experiments :—

1. I amalgamated half the surface of a strip of copper, and immersed it and a strip of zinc in water, acidulated with sulphuric or phosphoric acid ; on making the plates touch there was a rapid evolution of gas from the unamalgamated part of the copper, while only a few detached bubbles appeared on the amalgamated portion.

2. I placed a large globule of mercury (about half an ounce) in the bottom of a glass of acidulated water, and by means of a copper wire, the whole surface of which was amalgamated, made it communicate with one extremity of a

* I know of no other word to express the effect here alluded to ; the word is used in this sense by most French writers, but, from its numerous applications, is sadly inaccurate.

galvanometer, while a strip of amalgamated zinc immersed in the same liquid communicated with the other extremity; at the instant of communication an energetic current was indicated, which however immediately diminished in intensity, and at the end of a few minutes the needle returned to zero; scarcely any gas was evolved, and of the few bubbles which appeared as much could be detected on the surface of the zinc as of the mercury.

3. With the same arrangement I substituted for the mercury a strip of platinum well amalgamated. In this case, as before, after a few minutes the current became null, or so feeble as to require a delicate instrument to indicate its existence; and if, after the cessation of the current, the zinc was changed for unamalgamated platinum, this latter evolved torrents of hydrogen, and the needle indicated a violent current in a contrary direction.

4. With things arranged as in experiment 2, I employed a solution of sulphate of copper as an electrolyte instead of acidulated water; an energetic and constant current was produced, the mercury became amalgamated to saturation with reduced copper, and the precipitation of copper upon this amalgam continued as long as crystals of the sulphate were added to the solution.

By these experiments it appears that mercury, which in its normal state is well known to be inefficient as the positive metal of a voltaic combination, is in many cases equally inefficient as a negative metal, from its faculty of combining with the cations of the electrolytes, which, by rendering it equally positive with the metal with which it is voltaically associated, cause the opposed forces to neutralise each other. But if, as in experiment 4, the cation of the electrolyte is not of a highly electro-positive character, the zinc (or other associated metal) retains its superior oxidability and the voltaic current is not arrested. The application of these experiments to the phenomena presented by amalgamated zinc is evident; all the heterogeneous metals with which the zinc may be adulterated, and which form minute negative elements, being amalgamated, become by polarisation equally positive with the particles of zinc, and consequently without the presence of another metal

to complete the circuit, all action is arrested, as in the case of pure zinc. The fact of amalgamated zinc being positive with respect to common zinc, of its precipitating copper from its solutions, and other anomalies, are also explained by these experiments.

As in a common voltaic combination of zinc and mercury this effect is complicated by the variety of cations which are transferred to the negative metal, for instance, hydrogen, zinc, and alkaline matters,* I was anxious to discover whether hydrogen alone could, by its combination in small quantities with mercury, give it the same electro-positive qualities. Sir Humphry Davy, aided by a costly apparatus, scarcely succeeded in purifying water of alkaline matters, but the affinity of mercury for the alkaline metals gave me some hopes of attaining this object with less expensive means; to this end I submitted to electrolysation for five days, in a vessel of bees-wax, distilled water acidulated with pure sulphuric acid; the negative electrode, of amalgamated copper, terminating in a mass of mercury. At the end of this period I removed the mercury, substituting a fresh portion of the same metal which was perfectly pure, and renewed the electrolysation for two hours; I then quickly enclosed this last mercury in a tube with the purified water; it always evolved a small quantity of hydrogen, but I could not determine with certainty that its volume bore any given proportion to that of the mercury employed. Although this last portion of mercury when carefully cleaned and tested with reddened litmus-paper gave no alkaline reaction, yet its existence might be suspected as derived from the wax; and as metallic vessels were obviously objectionable, I sought other means of determining the part played by the hydrogen in this combination. I repeated experiment 2, keeping heated, below the boiling point, the vessel containing the zinc and mercury; my galvanometer gave a tolerably constant deflection of sixty degrees, and the mercury evolved much more hydrogen than when the apparatus was cold. Again, I kept for some time a strip of well-

* The current is not so completely null when dilute muriatic acid is the electrolyte, as it is with sulphuric or phosphoric acid; perhaps the sulphur or phosphorus contributes to the effect; the difference is, however, but trifling.

cleaned platinum in hydrogen gas, and then immersed it in mercury; when either platinum or mercury was moist I perceived a tendency to amalgamation, but none when they were perfectly dry. As I hope to renew this examination with a more perfect apparatus, I will not detail any more of these experiments, but merely state my impression to be that mercury under the influence of a voltaic current is capable of absorbing a very small quantity of hydrogen, which it gives up as soon as the communication is interrupted.

The probability of a temporary combination of hydrogen with mercury throws some light upon the movements of mercury submitted, under an electrolyte, to a voltaic current: the hydruretted particles of mercury are repelled until out of the immediate influence of the current, where they yield their hydrogen, and so on of the rest. In electrolysation with a mass of mercury as negative electrode, the hydrogen is all evolved at the parts most distant from the positive electrode.

In order to see whether this property of complete polarisation was proper to mercury, or common to all metals in a state of fusion, I caused two currents, one proceeding from a voltaic pair consisting of zinc and a fused globule of Darcet's alloy, the other from zinc and mercury kept at the same temperature, to pass in contrary directions through the wire of a galvanometer; the current proceeding from the first pair was much more energetic than that from the second, and kept the needle constantly at 85 degrees. I could not repeat the experiment with other metals, from the impossibility of keeping them fused without volatilising the electrolyte. As far as this case goes, it would seem that metals possess different polarising capacities. I have before remarked a difference in the proportionate diminution of the current by polarisation with solid metals, and think the subject merits an experimental examination.

ON A VOLTAIC PROCESS FOR ETCHING DAGUERREOTYPE PLATES.

From the *Proceedings of the London Electrical Society*, Part II.; having been read before the Society on August 17, 1841.

DR. BERRES, of Vienna, was the first, I believe, who published a process for etching Daguerreotypes: his method was to cover the plates with a solution of gum-arabic, and then to immerse them in nitric acid of a certain strength. I have not seen any plates thus prepared, but the few experiments which I have made with nitric acid have given me a burred and imperfect outline; and I have experienced extreme difficulty of manipulation from the circumstance of the acid never attacking the plate uniformly and simultaneously. My object, however, in this communication, is not to find fault with a process which I have never perhaps fairly tried or seen tried by experienced hands, and the inventor of which deserves the gratitude of all interested in physical science; but to make public another, which possesses the advantage of extreme simplicity, which anyone, however unskilled in chemical manipulation, may practise with success, and which produces a perfect etching of the original image; so much so, that a plate thus etched can scarcely be distinguished from an actual Daguerreotype, preserving all the microscopic delicacy of the finest parts of the impression.

One sentence will convey the secret of this process; it is to make the Daguerreotype the *anode** of a voltaic combination, in a solution which will not of itself attack either silver or mercury, but of which, when electrolysed, the anion will attack these metals unequally. This idea occurred to me soon after the publication of Daguerre's process; but, being then in the country, and unable to procure any plates, I

* Strictly speaking this is a misapplication of Faraday's term: he applied it to the surface of the electrolyte; as, however, all Continental and many English writers (among whom I may name Whewell) have applied it to the positive electrode, and as an expression is most needed for that, I have not hesitated so to apply it.

allowed the matter to sleep; and other occupations prevented for some time any recurrence to it. Recently having heard much conversation as to the practicability or impracticability of Daguerrotype engraving, I became anxious to try a few experiments in pursuance of my original notion; and for this purpose applied in several quarters for Daguerreotypes; but, thanks to the exclusiveness of M. Daguerre's patent, I found that to procure a sufficient number of plates for any reasonable chance of success was quite out of the question.

On mentioning the subject to Mr. Gassiot, he, with his usual energy and liberality, offered to procure me a sufficiency of Daguerreotypes; and it is owing to his zealous and valuable co-operation that I have been able to get such definite results as appear worth publication.

Five points naturally present themselves to the consideration of the experimenter on this subject: first, the quantity of the voltaic current; secondly, its intensity; thirdly, the distance between the anode and cathode; fourthly, the time during which the process should be continued; and fifthly, the solution to be employed.

1. With regard to the first element, or quantity, many previous experiments had convinced me that, to give the maximum and most uniform quantitative* action of any voltaic combination, the electrodes should be of the same size as the generating plates; in other words, that the sectional area of the electrolyte should be the same throughout the whole voltaic circuit. It seems strange that this point should have been so generally overlooked as it has been: an electrician would never form a battery, one pair of plates of which were smaller than the rest; and yet the electrodes, which, offering of themselves a resistance to the current from the inoxidability of the anode, are, *à fortiori*, a restriction when of small size, have generally been formed indefinitely smaller than the generating plates; I therefore, without further experiment, applied this principle to the process about to be detailed.

* I say quantitative action; for, where great intensity is required, as in decomposing alkalis, &c., it may be advisable to narrow the electrodes, so as to present a smaller surface for the reaction of the liberated elements.

2. *The intensity of the voltaic current.*—Here it appeared to me that, as in the electrotype, where the visible action is at the cathode, a certain degree of intensity throws down metal as a crystal, an increased intensity as a metallic plate, and a further intensity as a pulverulent mass; that degree of intensity which would show on the negative deposit the finest impressions from the cathode, would also produce on the anode the most delicate excavations, and consequently, an intensity which would just fall short of the point of evolving oxygen from the plate to be etched, would be the most likely to succeed; this point was not, however, adopted without careful experiment, the more so, as in one instance Mr. Gassiot succeeded in procuring a very fair etching with a series of ten pairs of the nitric acid battery; however, the results of repeated experiments, in which the intensity has been varied from a series of sixteen pairs to one of the nitric acid battery, were strongly in favour of the above idea, and consequently went to prove that one pair gives the most efficient degree of intensity for the purpose required.

3. *The distance between the plates.*—As it was proved by De la Rive that in an electrolytic solution, when the electrodes are at a distance, the action extends a little beyond the parallel lines which would join the bounds of the electrodes, and thus, that the current, as it were, diverges and converges, it appeared advisable to approximate the electrodes as nearly as possible, so as to produce uniformity of action over the whole plate. Provided a solution be used which does not evolve gas at the cathode, I am inclined to think that the plates may be with advantage indefinitely approximated; but as this was not the case with the solution I selected for the greater number of experiments, 0·2 of an inch was fixed on as the distance, in order that the gas evolved from the cathode should not adhere to the anode, and thus interfere with the action.

4. *Time of continuing the operation.*—This was a matter only to be decided by experiment, and must vary for the voltaic combination and solution employed. With a single pair of the nitric acid battery, from twenty-five to thirty seconds was, after a great number of experiments, fixed on as the

proper time; and as the plate may at any period be removed from the solution and examined, the first experiment should never exceed twenty-five seconds, when, if not complete, the plate may be again subjected to electrolysis.

5. *The solution to be employed.* — Here a vast field was open, and still is open to future experimentalists. Admitting the usual explanation of the Daguerreotype, which supposes the light parts to be mercury, and the dark silver, the object was to procure a solution which would attack one of these, and leave the other untouched. If one could be found to attack the silver and not the mercury, so much the better; as this would give a positive engraving, or one with the lights and shadows, as in nature; while the converse would give a negative one. Unfortunately, silver and mercury are nearly allied in their electrical relations. I made several experiments with pure silver and mercury, used as the anode of a voltaic combination; but found that any solution which would act on one acted also on the other. All, then, that could be expected was a difference of action. With the Daguerreotype plates I have used the following:—

Dilute sulphuric acid, dilute hydrochloric acid, solution of sulphate of copper, of potash, and of acetate of lead. The object of using acetate of lead was the following:—With this solution, peroxide of lead is precipitated upon the anode; and, this substance being insoluble in nitric acid, it was hoped that the pure silver parts of the plate, being more closely invested with a stratum of peroxide than the mercurialised portions, these latter would, when immersed in this menstruum, be attacked, and thus furnish a negative etching. I was also not altogether without hopes of some curious effects, from the colour of the thin films thus thrown down; here, however, I was disappointed: the colours succeeded each other much as in the steel plate used for the metallochrome, but with inferior lustre. On immersion in nitric acid of different degrees of dilution the plates were unequally attacked, and the etching burred and imperfect. Of the other solutions, hydrochloric acid was, after many experiments, fixed on as decidedly the best; indeed, this I expected, from the strong affinity of chlorine for silver.

I will now describe the manipulation which has been employed by Mr. Gassiot and myself, in the laboratory of the London Institution, with very uniform success. A wooden frame is prepared, having two grooves at 0·2 of an inch distance, into which can be slid the plate to be etched, and a plate of platinum of the same size. To ensure a ready and equable evolution of hydrogen, this latter is platinised after Mr. Smee's method ; for, if the hydrogen adhere to any part of the cathode, the opposite portions of the anode are proportionably less acted on. The back and edges of the Daguerreotype are varnished with a solution of shell-lac, which is scraped off one edge to admit of metallic connexion being established. The wooden frame, with its two plates, is now fitted into a vessel of glass or porcelain, filled with a solution of two measures hydrochloric acid, and one distilled water (sp. gr. 1·1) ; and two stout platinum wires, proceeding from a single pair of the nitric acid battery, are made to touch the edges of the plates, while the assistant counts the time ; this, as before stated, should not exceed thirty seconds. When the plate is removed from the acid it should be well rinsed with distilled water ; and will now (if the metal be homogeneous) present a beautiful sienna-coloured drawing of the original design, produced by a film of the oxychloride formed ; it is then placed in an open dish containing a very weak solution of ammonia, and the surface gently rubbed with very soft cotton, until all the deposit is dissolved ; as soon as this is effected it should be instantly removed, plunged into distilled water, and carefully dried. The process is now complete, and a perfect etching of the original design will be observed; this, when printed from, gives a positive picture, or one which has its lights and shadows as in nature ; and which is, in this respect, more correct than the original Daguerreotype, as the sides are not inverted. Printing can therefore be directly read ; and in portraits thus taken the right and left sides of the face are in their proper position. There is, however, *ex necessitate* this difficulty, with respect to prints from Daguerreotypes—if the plates be etched to a depth sufficient to produce a very distinct impression, some of the finer lines of the original must inevitably run into each other, and thus the chief beauty

of these exquisite images is destroyed. If, on the other hand, the process be only continued long enough to leave an exact etching of the original design, which can be done to the minutest perfection, the very cleaning of the plate by the printer destroys its beauty; and, the molecules of the printing-ink being larger than the depth of the etchings, an imperfect impression is produced. For this reason it appeared to me that at present the most important part of this process is the means it offers of multiplying indefinitely Daguerreotypes by means of the electrotype. An ordinary Daguerreotype, it is known, will, when electrotyped, leave a faint impression; but in so doing it is entirely destroyed, and this impression cannot be perpetuated; but one thus etched at the voltaic anode will admit of any number of copies being taken from it. To give an idea of the perfect accuracy of these, I may mention, that in one I have taken, on which is a signboard measuring on the electrotype plate 0·1 by 0·06 of an inch, five lines of inscription can, with the microscope, be distinctly read. The great advantages of the voltaic over the chemical process of etching appear to me to be the following:—

1. By the former, an indefinite variety of menstrua may be used; thus, solutions of acids, alkalies, salts, more especially the haloid class, sulphurets, cyanurets, in fact any element which may be evolved by electrolysis, may be made to act upon the plate.

2. The action is generalised, and local voltaic currents are avoided.

3. The time of operation can be accurately determined, and any required depth of etching produced.

4. The process can be stopped at any period, and again renewed if desirable.

The time I have given is calculated for experiments made with one pair of the nitric acid battery; it is, however, by no means necessary that this be employed, as probably any other form of voltaic combination would be efficient. It seems more advisable to employ a diaphragm battery, or one which produces a constant current, as otherwise the time cannot be accurately determined. It is very necessary that the silver of plates subjected to this process be homogeneous. Striæ, imperceptible in the original Daguerreotype, are instantly

brought out by the action of the nascent anion; probably silver, formed by voltaic precipitation, would be found the most advantageous. I transmit with this paper some specimens of the prints of the etched plates, and of electrotypes taken from them; and in conclusion would call attention to the remarkable instance which these offer of the effects of the imponderable upon the ponderable; thus, instead of a plate being inscribed, as 'Drawn by Landseer, and engraved by Cousins,' it would be 'Drawn by Light, and engraved by Electricity.'

[With this communication were sent plates etched by the process detailed in the text; electrotype copies from the same; and a considerable number of prints obtained from the former.—SEC. ELECT. SOC.]

Postscript by the Author, Nov. 1.—Few of the readers of the 'Philosophical Magazine' will have an opportunity of seeing any specimens of the process; and as the etching is not deep enough to produce impressions sufficient to accompany the paper, I may give an idea of them by saying that in the print of a portrait which I have now before me the whole expression of the features is distinct, the pupil of the eye and the speck of light upon it clearly defined, the gloss of the hair and of the satin stock very accurate. The microscopic details alone appear incapable of transference to *paper*; but these, as stated above, being *absolutely perfect* upon the etched plate, I had intended to have directed some experiments to the substitution of more delicate materials than paper and printing-ink for receiving the impressions; incessant occupations have prevented me, and will, I fear, for some time.

I would suggest the employment of hyposulphite of soda instead of ammonia to remove the oxychloride.

ON SOME ELECTRO NITRURETS.

Proceedings of the Royal Society, Feb. 4, 1841.

THE author states that he has made many attempts to render permanent the ammoniacal amalgam, and that he has succeeded in freezing it by means of solid carbonic acid, during

which solidification, and also while in its solid state, it underwent no chemical change. He subsequently attempted to procure a permanent compound by electrolysing a solution of hydrochlorate of ammonia with an extremely fusible alloy at the cathode; but this attempt was unsuccessful. It then occurred to him, that by using an oxidable metal at the anode, which could be revived in conjunction with nascent hydrogen and nitrogen at the cathode, one or both of these elements might be combined with the solid metal and so form permanent compounds.

The experiment made in this manner with the metals zinc, cadmium, and copper was perfectly successful. A spongy mass collected at the cathode, which floated upon the liquid, and which, when washed and dried, was analysed by heating in a tube retort; five grains of the zinc compound gave 0·73 of a cubic inch of permanent gas, which on examination proved to be nitrogen with one-fourth hydrogen. The same quantity of the cadmium compound gave 0·207 cubic inch of nitrogen, with no admixture of hydrogen. A like weight of the copper compound gave 0·107 of nitrogen. No ammonia was evolved from either; and the author is inclined to think that the hydrogen yielded by the zinc compound resulted from the reaction of the metal upon combined water. The specific gravity of specimens of these substances which the author tried were respectively 4, 6, 4, 8 and 5, 9. A mixed solution of chloride of gold and hydrochlorate of ammonia, electrolysed with platinum electrodes, gave a black powder of the specific gravity 10·3, five grains of which, being heated, gave only 0·05 cubic inch of gas. The author proceeds to observe, that the similarity in appearance and mode of formation of these compounds and of the mercurio-ammoniacal amalgam is strong evidence of identity of constitution, and that the non-permanence of the latter substance is due to the mobility of the mercury; for if we place the compounds in similar circumstances, that is, solidify the mercurial one, or liquefy those of the other metals, the phenomena are perfectly analogous. The experiments also bear immediately upon those of Thénard, Savart, and others, where ammonia, passed over heated metals, was found to be decomposed more completely

by the oxidable than by the inoxidable metals, and to alter their physical characters without materially increasing their weight. On examining papers connected with this subject the author found that Mr. Daniell had cursorily noticed a deposit somewhat analogous to those here treated of, which was formed upon the negative plate of his constant battery, when this was charged on the zinc side with hydrochlorate of ammonia, and the nature of which that gentleman observed was worthy of further examination, but had not had time to investigate.

SYNTHESIS OF WATER BY VOLTAIC ACTION.

Philosophical Magazine, Feb. 1839.

Two strips of platinum, two inches long and three-eighths of an inch wide, standing erect at a short distance from each other, passed, hermetically sealed, through the bottom of a bell glass; the projecting ends were made to communicate with a delicate galvanometer; the glass was filled with water acidulated with sulphuric acid, and both the platina strips made the positive electrodes of a voltaic battery until perfectly clean, &c.; contact with the battery having been broken, over each piece of platinum was inverted a tube of gas, four-tenths of an inch in diameter, one of oxygen, the other of hydrogen, acidulated water reaching a certain mark on the glass, so that about half of the platina was exposed to the gas, and half to the water. The instant the tubes were lowered so as to expose part of the surfaces of platinum to the gases the galvanometer needle was deflected so strongly as to turn more than half round; it remained stationary at 15°, the platinum in the hydrogen being similar to the zinc element of the pile. When the tubes were raised so as to cover the plates with water, the needle returned slowly to zero; but the instant that the tubes were lowered again, it was again deflected; if the tubes were changed with regard to the platina, the deflection was to the contrary side.

The action lowered considerably after the first few minutes, but was in some degree restored every time the tubes were raised so as to wash the surface of the platina, and again lowered. After twenty-four hours the water had risen half an inch in the tube containing hydrogen, and three-eighths of an inch in that containing oxygen. In two other tubes, without platina, but with the same gases, and immersed in acidulated water for the same time, the water had scarcely perceptibly risen; the effect therefore could not have been due to solution. The same sheets of platinum were exposed to atmospheres of common air and of similar gases, *i.e.* both to oxygen or both to hydrogen, &c., but without affecting the galvanometer. The platinum in the hydrogen was made the positive, and that in the oxygen the negative electrode of a *single* voltaic pair; the water now rose at the rate of three-eighths of an inch per hour in the hydrogen tube, and proportionally in the oxygen; when the platina was not assisted by a pair of metals the oxygen was absorbed in more than its relative proportion. I hope, by repeating this experiment in series, to effect decomposition of water by means of its composition.

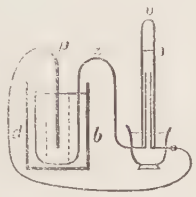
EXPERIMENTS ON VOLTAIC REACTION.

On the weekly evening meeting of the Royal Institution for March 13, 1840, I communicated some experiments and observations on certain phenomena which I collated under the general term Voltaic Reaction. I then stated, that in certain (probably in all) cases of the development of a voltaic current a reaction was induced by the voltaic force itself, and that upon the cessation of the initial force the reacting force was apparent in an opposed direction. I showed, moreover, that the diminution or removal of this reaction was one means of increasing the power of the initial current. This reaction in electrolytes (though it is by no means confined to electrolytes) is what has been generally called polarisation.

It recently occurred to me, that as one method of increasing the power of the initial current was to diminish (or, as it were,

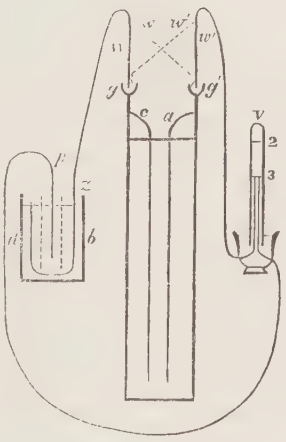
absorb) this reaction, so another method of effecting the same object would be, to add the reacting to the initial force, which, from the separable character of the former, did not appear impracticable. After sundry devices the following experiments realised my views on the subject.

Fig. 1.



Experiment 1, fig. 1.—*d b* is a single cell of the nitric acid battery, exposing six square inches of each metal; *v* is an ordinary voltameter, each electrode exposing half a square inch, charged with dilute sulphuric acid; decomposition was allowed to proceed with this arrangement for six hours; the battery, for greater assurance of constancy, being in this and the two following experiments re-charged every two hours; the level of the liquid in the voltameter was carefully marked on the tube.

Fig. 2.



Experiment 2, fig. 2, is the *same* nitric acid battery, *d b*, the *same* voltameter, *v*, but with an interposed pair of large platinum plates, *a c*, exposing each to each forty-two square inches of surface, and immersed in dilute sulphuric acid; this arrangement was also set to work for six hours. A slight evolution of gas had taken place in the voltameter in this experiment, and the water-level was also marked.

Experiment 3.—The same apparatus as fig. 2; but my assistant was directed to change at a certain interval the wires dipping into the mercury cups, *g g'*, so as to reverse the plates, *a c*, with regard to the direction of the current, making what was the anode the cathode, and *vice versâ*, as shown by the dotted lines; and at the expiration of a similar interval to restore them to their original positions, and to continue thus alternating the position of these plates with reference to the current during six hours.

The interval was to be dependent upon the following observation :—When first the circuit was completed a marked evolution of gas was perceptible in the voltameter ; this gradually subsided, and when it had become nearly imperceptible the change was to be made, when a fresh burst of gas took place ; as this again subsided the wires were to be again changed, and so on. At the expiration of six hours the water-level was marked, as in the previous experiments.

The following is the quantity of gas evolved in the voltameter, deduced from a mean of several experiments :—

Cubic inch.

Experiment 1. = 0·15
Experiment 2. = 0·10
Experiment 3. = 0·23

In neither of the last two experiments was a bubble of gas perceptible on the large plates, *a c*.

It appears from these experiments that the nitric acid battery will decompose water across two pairs of interposed inoxidable electrodes, provided one be of considerable size with reference to the other parts of the circuit, so as to lessen resistance.

Whether this diminished resistance be occasioned by the mere increase of the sectional area of the electrolyte ; whether by the increased facility for solution of the oxygen and hydrogen ; whether the oxygen and hydrogen be not eliminated, but merely thrown into a state of polar tension, or made to adhere in a liquid or gaseous form to the plates ; or whether any of these effects take place conjointly, I will not stop to enquire, but proceed to the more remarkable fact, viz. that the quantity of gas evolved in Experiment 3 is greater for a given time, not only than that evolved in Experiment 2, but even than that evolved in Experiment 1 ; thus we get the seeming paradox, that a battery performs more work with an interposed resistance than without it.

While the battery is decomposing water in the voltameter *v*, it is polarising the plates *a c*, or accumulating by its own force an antagonist force ; when the wires are changed this reacting force is united in direction with the initial force ; in

fact two voltaic pairs are constituted. The reaction being exhausted, a new polarisation commences, to be added in its turn to the initial current; and the reason why we get the increased work at the voltameter is, that *while* the polarisation is proceeding at *a c*, water is decomposed in the voltameter; and although this may be somewhat less than the battery would produce without the interposed plates *a c*, still this deficiency is more than made up by the action of the double pair at each alternation of the wires. If the view I have taken be correct, as reaction can never be greater than the action which occasions it, we should never get, in Experiment 3, beyond the quantity of gas given by two pairs of the battery *d b*, but we may indefinitely approach that maximum.

A commutator might be easily arranged instead of the hand for effecting the alternation at the proper periods, which, by a little contrivance, may be made to work by the battery itself, but I prefer stating the experiment in its most simple form, and free from mechanical complication.

Although the experiment as here described is merely in illustration of a principle, it appears to me to promise results of some practical value. The economy of this method of applying force is evident: we get all but a double product with a single consumption. The principle in all probability is not confined to the voltaic force, but may perhaps be applied to mechanics.

ON THE GAS VOLTAIC BATTERY.

Philosophical Transactions of the Royal Society. Received March 27. Read May 11, 1843.

In the 'Philosophical Magazine' for December 1842 I have published an account of a voltaic battery in which the active ingredients were gases, and by which the decomposition of water was effected by means of its composition.

The battery described in that paper consisted of a series

of tubes containing strips of platinum foil covered with a pulverulent deposit of the same metal; the platinum passed through the upper parts of the tubes, which were closed with cement, the lower extremities were open; they were arranged in pairs in separate vessels of dilute sulphuric acid, and of each pair one tube was charged with oxygen, the other with hydrogen gas, in quantities such as would allow the platinum to touch the dilute acid; the platinum in the oxygen of one pair was metallically connected with the platinum in the hydrogen of the next, and a voltaic series of 50 pairs was thus formed. With this battery the following effects were produced:—

1. A shock was given which could be felt by five persons joining hands.

2. The needle of a moderately sensitive galvanometer was whirled round and remained permanently deflected 60°.

3. A gold-leaf electroscope was notably affected.

4. A brilliant spark visible in broad daylight was given between charcoal points.

5. Iodide of potassium, hydrochloric acid, and water acidulated with sulphuric acid were severally decomposed; the gas from the decomposed water was collected and detonated. The gases were evolved in the direction which the chemical theory would indicate, the hydrogen travelling in one direction thoughout the circuit, and the oxygen in the reverse.

When distilled was substituted for acidulated water in the battery cells the effects were similar but more feeble.

The effects, though clear and decisive in themselves, were further tested by counter-experiments, such as reversing the current by reversing the gases, &c.; but these I need not here detail, as the electrical effects of the gas battery, when charged with oxygen and hydrogen, have since the publication of that paper been repeatedly verified. I farther stated, that when carbonic acid and nitrogen were substituted for oxygen and hydrogen no voltaic effects were produced; that oxygen and nitrogen produced no effects, but that hydrogen and nitrogen did produce a voltaic current, which I attributed to the combination, with the hydrogen, of the oxygen of atmospheric air in solution. This opinion will be further tested in the following paper.

The voltaic current generated by this battery I attributed to chemical synthesis, of an equal but opposite kind, in the alternate tubes, at the points where the liquid, gas, and platinum met, and the object of covering the platinum with the pulverulent deposit* was to increase the number of these points, the liquid being retained upon the surface of the platinum by capillary attraction.

The point which appeared to me at that time as most important was the beautiful instance of the correlation of natural forces exhibited by the fifth effect, in which gases by combining and becoming a liquid transfer a force which is capable of decomposing a similar liquid, and causing its constituents to become gases, heat, chemical action, and electricity being all mutually dependent.

The apparatus with which I made the above experiments being composed of some pieces of tubing which happened to be in my laboratory, did not enable me to attain any precise accuracy of measure as to the volumes of gases absorbed, or to prove that Faraday's law of definite electrolysis finds no exception in the gas battery. Since that paper was written I have, after some failures, constructed apparatus by which I have been enabled to verify this law and to extend my researches into the nature of gaseous voltaic action. I have felt the more called on to multiply experiments on this subject, as a letter has been published on the gas battery, written by an electro-chemist for whose opinion I have much respect, which attributes its action to a cause different from that to which I assigned it.

Soon after my original publication I received a letter from Dr. Schœnbein, the substance of which has since appeared in print.† Dr. Schœnbein there expresses an opinion that in the gas battery oxygen does not immediately contribute to the production of the current, but that it is produced by the combination of hydrogen with water. I have recently heard a similar opinion to that of Dr. Schœnbein expressed by other

* For the method of effecting this see Mr. Smee's paper, *Phil. Mag.*, April 1840.

† *Phil. Mag.*, March 1843, p. 105.

philosophers, but I must take the liberty of dissenting from it and of adhering to that which I expressed in my original paper. My grounds of dissent will be seen in some of the ensuing pages.

In describing the apparatus used in the following experiments I shall mention three forms of gas battery, with the first two of which my experiments were all performed; the latter has only occurred to me while writing this paper. I have, therefore, not yet had an opportunity of trying it,* but it appears to me by far the best of the three, though, doubtless, superior modifications will shortly be discovered.

Fig. 1. Fig. 2. Fig. 3.

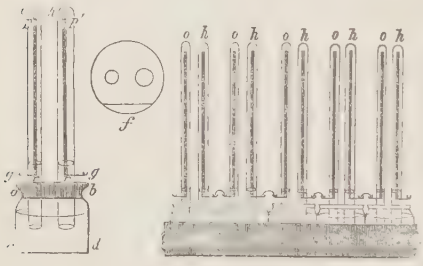


Fig. 1, represents one of these forms; *a, b, c, d* is a wide-mouthed glass jar, into which a wooden plug, *a, b*, fits tightly by means of attached pieces of cork; this wooden cover is perforated to receive the tubes *o, h*, of which the size is such that the content of *h* shall be double that of *o*, and which are firmly cemented into it; the wooden cover is shown in plan in fig. 2; the piece *f* is capable of being detached at pleasure, in order to introduce a tube for charging the apparatus with gas; *p r, p' r'* are strips of well-platinised platinum foil, slightly curved like a cheese-scoop to keep them erect and in the centre of the tube, and riveted or welded to stout platinum wires, which are hermetically sealed into the glass, and terminate in brass mercury cups at *g, g*. This form of battery is charged by inverting it so as to fill the tubes with liquid; on re-inversion the tubes may be charged with gas from a crooked tube and bladder. The apparatus (fig. 1.) is represented as charged and ready for use; and in fig. 3 is a battery of five cells, also represented as just charged.

The advantage of this form over that which I shall next describe is the facility with which the tubes are filled with

* See Postscript.

liquid, and the absence of any necessity of touching the electrolyte with the fingers. On the other hand, its disadvantages are the difficulty of examining the gases after experiment, and the impossibility of doing so during experiment without changing the electrolyte, as in order to examine the gases the whole apparatus must be immersed in a water-trough, and the cover with the attached tubes taken off while the jar and the ends of the tube are under water.

Fig. 4 represents a cell of the second form; *b*, *c*, *d*, *c* is a parallelopiped glass or stoneware vessel, such as is commonly used for the outer cells of nitric acid batteries; the tubes are cemented into pieces of wood, *a b*, *a c*, and can with the wood be separately detached from the trough, as shown in fig. 5. At the aperture or space, *a a*, between the tubes there is just room for a finger to enter, close the orifice of either tube, and thus detach it from the apparatus. In this figure the platinum foil is turned up round the edge of the tube, instead of being attached to a wire sealed into the glass, and instead of a mercury-cup there is a binding-screw connexion; but it is obvious that this part of the arrangement may be interchanged with the other apparatus, or varied *ad libitum*. This apparatus I have found in practice to be very much more convenient than the former, from the facility of detaching either tube so as to discharge some of the gas, if it be desirable to alter the level of the water-mark; or to examine or change the gas in any of the tubes. On the other hand, it has the disadvantage of requiring the finger to be immersed in the electrolyte, which, when the latter is of an active chemical character, is unpleasant and in some cases injurious. In fig. 6 a battery of five cells of this construction is represented as when charged with oxygen and

Fig. 4. Fig. 5.

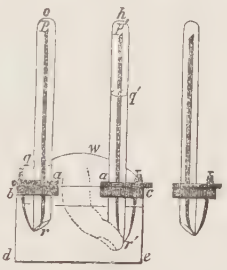
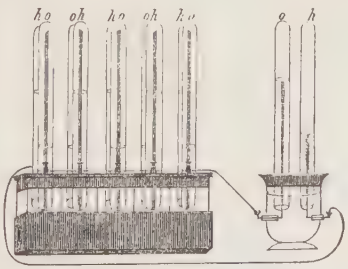


Fig. 6. Fig. 7.

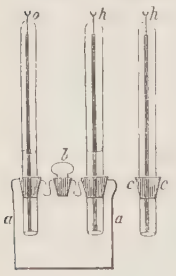


hydrogen, and having been for some time connected with a voltameter (fig. 7), the tubes of which are of the same size as those of the battery.

In the form last described (figs. 4 and 6) the tubes were all as nearly of the same size as could be procured ; they contained each about one and a half cubic inch ; in the first form (figs. 1 and 3) the portion *o*, *r* of the narrow tube contained one and a quarter cubic inch, and the portion *h*, *r'* of the wide tube contained two and a half cubic inches. A portion of the apparatus with which I wrought was constructed by my order for the London Institution, and another portion belonged to Mr. Gassiot, and was by him very kindly placed at my disposal for the purpose of these experiments. Had it not been for this valuable addition, I should have been obliged to make all my experiments on a much smaller scale ; they would have taken more time and been by no means so satisfactory.

As I have already stated, a third form has occurred to me while writing this paper, which I think in many respects more advantageous than either of the two preceding, and which, as it may be some time before I can experiment with it myself, I will here describe for the benefit of those who are differently situated. One cell of it is shown in fig. 8 : *a*, *a*, is a Woulfe's bottle with three necks ; in the centre neck is fitted a glass stopper, *b* ; in the other two tubes *o*, *h* fit accurately by means of glass collars (*c c*, fig. 9.) welded to them, and ground on the outside ; the platinum is hermetically sealed into the tops of the tubes, which may be charged in a similar manner to fig. 1. By immersing this apparatus in the water-trough each tube with the gas it contains may be detached and examined separately ; but its principal advantage is, that by slightly greasing the stopper and collars it may be made perfectly air-tight, which, for reasons that will be apparent in the course of this paper, is a most material point. This apparatus, moreover, being entirely composed of glass and platinum, concentrated acid, alkaline or other corrosive solutions may

Fig. 8. Fig. 9.



be used as the electrolyte, without damaging the apparatus or introducing foreign matter.

In the experiments I am about to describe the results were generally tested by chemical action, as manifested by the electrolysis, either of iodide of potassium or of water. I had at my disposal a highly sensitive astatic galvanometer; but I found such slight local actions disturb it, that a range of test experiments was in each case necessary to eliminate the true battery action from the accidental currents; and, with all the pains that could be bestowed upon it, the results were less definite and trustworthy than those obtained with the iodide.

I may here state also that, although with the battery described in my original paper, when charged with oxygen, hydrogen, and dilute sulphuric acid, I could not succeed in perceptibly decomposing water with less than twenty-six cells, yet the new arrangements, from their superiority in size and construction, were capable, when charged with the same gases and electrolyte, of decomposing water with four cells; and a single cell would decompose iodide of potassium.

Experiment 1.—Ten cells charged to a given mark on the tube with dilute sulphuric acid, sp. gr. 1·2, oxygen and hydrogen, were arranged in circuit with an interposed voltameter,* as in figs. 6 and 7, and allowed to remain so for thirty-six hours. At the end of that time 2·1 cubic inches of mixed gas were evolved in the voltameter; the liquid had risen in each of the hydrogen tubes of the battery to the extent of 1·5 cubic inch, and in the oxygen tubes 0·7 cubic inch, equalling altogether 2·2 cubic inches; there was therefore 0·1 cubic inch more of hydrogen absorbed in the battery tubes than was evolved in the voltameter.

This experiment was several times repeated, with the same general results. I give some of them in the annexed table.

* These experiments were made with the battery fig. 1, though for more clearly showing the volumes of the gases the second form is represented in figs. 6 and 7. The voltameter employed on this occasion had electrodes of fine platinum wire a quarter of an inch long. From the nature of the gas battery it is difficult to know the efficient surface of the plates. In ordinary batteries I have found, and stated some time ago, that for quantitative effects the electrodes should be of the same size as the battery plates.

Cubic inch of oxygen absorbed in the battery cells	Cubic inch of hydrogen absorbed in the battery cells	Cubic inch of oxygen evolved in voltameter	Cubic inch of hydrogen evolved in voltameter	Time	Number of cells
0·7	1·4	0·7	1·4	36 hours.	10
0·5	1·2	0·5	1·1		
0·6	1·4	0·6	1·3		
0·6	1·3	0·5	1·2		
0·6	1·4	0·6	1·3		
Mean 0·6	1·34	0·58	1·26		

We may observe generally, in these experiments, that the hydrogen evolved in the voltameter is somewhat more than double the volume of the oxygen, and that a still extra quantity of hydrogen is absorbed in the battery. With regard to the excess of hydrogen in the voltameter, this, as is well known to electricians, is always observable in the electrolysis of water, and has been attributed by Faraday to the more ready solubility of oxygen, and its tendency to form oxygenated water;* but we have in the above experiments a still greater excess of hydrogen absorbed in the battery tubes. This result previous experiments had led me to expect. In one of these I found voltaic action produced by tubes charged alternately with hydrogen and water, and attributed it to the combination of hydrogen with the oxygen of atmospheric air in solution.† Assuming for the moment this explanation to be correct, in a gas battery charged with oxygen and hydrogen we should have, upon completion of the circuit, three distinct voltaic actions:—First, the principal action occasioned by the gases in the tubes reacting upon each other through the medium of the electrolyte, *i.e.* reverting to fig. 4, an action in which the portions of the platinum exposed to the gases $p\ q, p'\ q'$ would be the efficient plates. Secondly, an action between the hydrogen at $p'\ q'$ and the air in solution in the neighbourhood of the immersed portion of the plate, q, r; this would add to the general current, but would tend disproportionately to diminish the hydrogen. Thirdly, a local action between the

* *Experimental Researches*, § 716, 717.

† *Phil. Mag.*, Dec. 1842, p. 419, Exp. 11.

hydrogen at p', q' and the air in solution around the part q', r'; this would add nothing to the general current, but would also tend to diminish the hydrogen. As this last is totally independent of the general action, it could be abstracted by merely placing a cell charged similarly to the battery out of the circuit with the terminals unconnected, as in fig. 1. In a cell so placed the hydrogen was found to be absorbed in the ratio of rather less than 0·1 cubic inch in twenty-four hours.

On some occasions I found the rise of liquid in the hydrogen cell to be unequal in different tubes of the battery, and this I found more particularly the case in the battery fig. 4. It was some time before I discovered the cause of this. I will not enumerate my different conjectures, but state that which proved to be the correct one. As, in using the two forms of batteries (figs. 1 and 4), the chief difference consisted in the introduction of the finger, it occurred to me that my assistant's hands, which were employed in various manipulations, might, in placing the tubes in the cells of fig. 4, introduce into the electrolyte small portions of foreign matter, particularly metals, and that thus a local action might be occasioned; this view was strengthened by my frequently observing copper deposited upon some of the immersed portions of the platinum, and where this happened an excess of hydrogen was generally found to have been absorbed. To examine the accuracy of this view, in

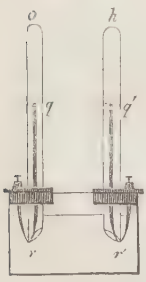
Experiment 2, I caused four cells to be charged with a solution of sulphate of copper, and connected in closed circuit; after twenty-four hours' work, the liquid in the oxygen and hydrogen tubes had risen equally in three of the pairs, but in the fourth the liquid in the hydrogen tube had risen rather more than twice as high as in any of the others, and the whole of the platinum in this tube, from the water-mark downwards, was covered with metallic copper; it was thus evident that a slight precipitation having commenced on this platinum from some local circumstance, which offered less resistance in this cell than in the others, a separate local current had been established; the hydrogen and the copper acting as a voltaic circuit, fresh copper had been constantly deoxidated at the expense of the hydrogen. The phenomenon is analogous

to that observable in an ordinary sulphate of copper battery, when a slight portion of copper is deposited upon the zinc, and a local current is established by which the zinc is worn into a hole without contributing to the general current.

I have been thus particular in order to explain points in the action of this battery which might seem exceptions to the law of definite electrolysis, or what perhaps we should here call electro-synthesis. As a general result the equivalent action of the battery was very beautiful; with fifty cells in action there was but a trifling difference in the rise of liquid in all the cells; and the rise of gas in the voltameter appeared so directly proportional, that an observer unacquainted with the *rationale* of a voltaic battery would have said the gases from the exterior cells of the battery were conveyed through the solid wires and evolved in the voltameter; and had this been the first voltaic battery ever invented, this probably would have been the theory of its action.

In my original paper I considered the points of voltaic action to be those where the liquid, gas, and platinum met; and it was to increase the number of these points that I employed platinised or spongy platinum; indeed, from what I have since observed, I have much doubt whether I should have obtained any success had I used smooth platinum. The local action detailed in the last experiment, however, made me anxious to ascertain whether the principal points of action were those which I had originally believed, or whether the gases entered into solution first, and were then electro-synthetically combined by the immersed portion of the platinum; whether, for instance, the efficient parts of the plates were the parts *p q*, *p′ q′* (fig. 4), or *q r*, *q′ r′*. To ascertain this the following experiment was made:—

Fig. 10.



Experiment 3.—A battery of five cells was constructed, in which the platinum reached only to half the height of the tubes (see fig. 10). This was charged with oxygen and hydrogen, so that the liquid just covered the extremities of the platinum. In this case we have only the immersed portions of the platinum, *q r*, *q′ r′*, and can examine the action

of the gases which enter into solution, and are unaffected by the platinum until in solution. This battery so charged gave a very trifling action indeed; it would not decompose iodide of potassium, and but slightly affected a highly sensitive galvanometer; but when a little gas was added, so as to expose the platinum to the gaseous atmosphere, a considerable current was developed, and a single pair decomposed the iodide.

If, again, a battery of this description (fig. 10) be charged so that the water-mark is below the upper edge of the platinum, and the ends are connected in closed circuit, the liquid rises in both tubes until that in the hydrogen tube has reached the top of the platinum, and then there is no farther rise. This experiment decides the question as to what is to be considered the working portion of the battery, but it does not positively decide whether solution and electrolysis are contemporaneous or successive, as it may be said that even what I have termed the exposed parts of the platinum are covered with a film of liquid. I should myself hesitate for the present to express a decided opinion on this point; my first impression was, that there would be, as it were, three sets of points in contact, but I have not been able to devise an experiment definitely to settle this point.*

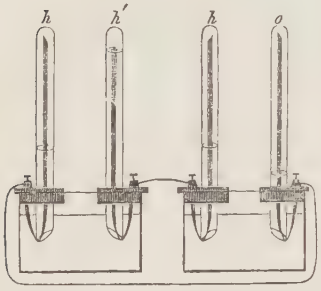
I aimed next at farther establishing the analogies of this battery with the ordinary voltaic battery, *i.e.* regarding the hydrogen tube as analogous to the plate of zinc or other oxidable metal at the anode. I wished to see how far this relation was borne out. It was beautifully shown in

Experiment 4, where a single pair was charged with oxygen and hydrogen, and a second with hydrogen in one tube, the other being filled with dilute sulphuric acid; when the hydrogen of the second was metallically connected with the oxygen of the first, and the liquid of the second with the hydrogen of the first, as in fig. 11, bubbles of gas rose from the platinum, which proved, as I anticipated, to be hydrogen. In short, though it required four pairs to decompose water

* I have sometimes remarked, that when mixed oxygen and hydrogen have been collected in one tube of the gas battery over distilled water, the addition of a little sulphuric acid causes the gases rapidly to disappear.

with immersed platinum electrodes, yet the platinum in the atmosphere of hydrogen being analogous to an oxidable anode, one pair was, with this assistance, sufficient to decompose water, just as one pair of an ordinary battery will decompose water with an anode of copper.

Fig. 11.



The nitric acid battery, an account of which I originally published in 1839, having shown me the value of highly oxygenated acids and peroxides as voltaic excitants,* I determined, with a view of farther extending the analogy of the gaseous and metallic voltaic batteries, to try the nitric acid as an electrolyte with the gas battery. Therefore, in

Experiment 5, I charged a battery with hydrogen and nitric acid in alternate cells, the nitric acid being only diluted sufficiently to prevent injury to the wooden parts of the battery. With this arrangement I found that three cells were capable of decomposing water; and thus here, also, the analogy held good, the gaseous hydrogen deoxidating the nitric acid in this arrangement, by nascent hydrogen as in the metallic battery.

I now endeavoured to produce the converse effects, viz. to form a battery in which oxygen should be the gaseous element, and be absorbed by an electrolyte having an affinity for it. To this end, in

Experiment 6, I caused a battery of ten cells to be charged, the one set of tubes with oxygen and the alternate tubes with solution of protosulphate of iron. This battery decomposed iodide of potassium, but was not able to decompose water. The tubes which contained the solution of protosulphate represented the hydrogen tubes of the ordinary gas battery. The voltaic action caused by oxygen and protoxide of iron was, however, but temporary.† After a few hours it

* See *Phil. Mag.*, May and Oct. 1839, pp. 389, 290.

† In Experiment 26 it will be seen that a *continuous* current is obtained from oxygen and a liquid (solution of ammonia); oxygen likewise gives a current with solution of cyanogen, and probably with many organic compounds.

abated, the iodide was no longer decomposed, and the liquid did not rise perceptibly in the tubes containing oxygen. The solution when tested by ferrocyanide of potassium gave a blue precipitate, indicating the presence of peroxide, but the greater portion of this was probably formed at the expense of the atmospheric air.

In the last experiments and others I had observed that a more decided effect was obtained when free hydrogen alone was present than when oxygen was alone. In my former paper I attributed this to the atmospheric air in solution; and for convenience of arrangement I have hypothetically assumed this explanation in the commencement of this paper, but the recent letter of Dr. Schœnbein induced me to look farther into this point. Therefore, in

Experiment 7, I charged two batteries, of two cells each, with hydrogen and dilute sulphuric acid in the alternate cells. When tested by iodide of potassium each battery gave notable effects. One of these batteries was then placed, together with a cup containing phosphorus, in a shallow vessel of water; the phosphorus was ignited and a large glass vessel inverted over the whole; the terminal wires of the battery, carefully protected by thick coatings of cement, passed under the edge of this vessel through the water, the exterior surface of which was covered with oil, more effectually to prevent the absorption of air. The terminal wires were then united and left so. After two hours, when the oxygen of the surrounding atmosphere had been exhausted by the phosphorus, the effect became more feeble, but continued throughout the evening. The next morning, however, the enclosed battery produced not the slightest effect upon the iodide; the liquid had risen in the hydrogen tubes about 0·2 cubic inch, but no other effect was perceptible. On the other hand, in the battery which had been placed by its side, charged in the same way, and similar in every respect but in the fact of being exposed to the atmospheric air, a very decided effect was produced; hydrogen had been evolved from one of the platinums to the extent of 0·3 cubic inch in the cell containing liquid, and a decided effect was produced on the iodide. The two batteries were left in this state for three more days; the decomposition and the evolution of hydrogen continued

in the exposed battery, but none was perceptible in the inclosed one, although the liquid had risen a little more, viz. 0·1 cubic inch, in the hydrogen tubes of the latter. After the four days above mentioned the jar of nitrogen which covered the battery was taken away, and the action of the battery was tested by iodide of potassium. At first there was no action, but after about fifteen minutes a slight action was perceptible; this gradually increased, and in two hours the action was equal to that of the battery which had been from the first exposed to the atmosphere. I cannot but regard this experiment as a conclusive negation of that view which regards hydrogen and water as the efficient agents in the gas battery. The opinion appears to me to have arisen from the circumstance of our working always in an atmosphere containing oxygen, and also from the fact of this latter gas being more soluble than hydrogen.* If we lived in an atmosphere of hydrogen, and if this gas were equally or more soluble than oxygen, I have little doubt that the converse effects would be observed. A battery charged with hydrogen in one set of tubes and acidulated water in the alternate ones, at first gives an effect nearly equal to an oxyhydrogen gas battery, but the action rapidly declines in the former, while it is constant in the latter. Even the ordinary action of the gas battery when charged with oxygen and hydrogen appears to me unanswerable as to the point I am now discussing. When we see a battery of a number of cells at work, and the liquid gradually rising in the oxygen tubes, just in the proportion in which oxygen gas is eliminated in the voltameter, and when in a similar battery placed by its side, similarly charged, but not connected in closed circuit, not the slightest rise takes place in any tube, it seems impossible to adopt the conclusion that the oxygen has nothing to do with the current. Here we have no slight galvanoscopic effects, but chemical effects capable of quantitative admeasurement, capable of being continued to an extent only limited by the size of the apparatus, and equivalent to the chemical effects

* The tendency of oxygen to combine with platinum may also have its influence. See M. De La Rive's various experiments on this subject, *Bibl. Univ. passim.*

observable at the voltameter. If, on the other hand, hydrogen and water be the only active elements, what becomes of the hydrogen? If it combine with the water, we undoubtedly should by this means be able to obtain a suboxide of hydrogen,* a result of which I have not seen the slightest symptom in a long course of experiments on this subject. Even if we assume the action of the oxygen to be a depolarising one, as suggested by Dr. Schœnbein, this comes to the same thing, as this depolarisation can only be accounted for as being effected by the combination of the oxygen with hydrogen; and we might conversely assume this combination to be the efficient cause of the current, and the depolarisation to take place in the hydrogen tubes. It seems that the effects at both anode and cathode are reciprocally dependent. The matter appears to me so clear that I should not have entered into detail upon it were it not for the published letter of Dr. Schœnbein above mentioned, and that the superiority of the hydrogen is *primâ facie* very striking. Knowing also the fondness with which we all adhere to preconceived opinions, as the consideration of the action of spongy or clean platinum on *mixed* gases led me to the discovery of the gas battery, I felt that I might be too apt to measure the correctness of my opinions by the success of the experiments to which they led, and therefore hesitated too confidently to rest upon what appeared to my mind positive demonstration.

Having verified the *rationale* of the action of the gas battery, I now sought to extend it to other gases, and caused arrangements of ten cells to be charged with such gases as were sufficiently insoluble to remain in the tubes time enough for experimental investigation. In all the following experiments, besides the ten cells charged in series, a single cell charged with similar gases and electrolyte was placed by the side, but with the terminals unconnected: thus, when the battery circuit had been closed for some time, by comparing the changes which had taken place in the battery tubes with those in the detached and unconnected pair, the effects due to

* I see by a recent paper of Dr. Schœnbein that he believes this to be the case.—*Archives de l'Electricité*, No. 7, p. 73.

solution, local currents, or other causes could be abstracted from those due to circulating voltaic action.

I shall arrange the following experiments in the order in which I instituted them, making such comments as may be necessary to explain my own deductions from the resulting phenomena. When not otherwise mentioned, the electrolyte will be considered as dilute sulphuric acid, sp. gr. 1·2.

Experiment 8.—A battery charged with oxygen and protoxide of nitrogen produced no effect upon iodide of potassium. Examined next day, the liquid had not risen in the oxygen tubes; in the protoxide tubes it had risen to an average of 0·3 cubic inch, both in the battery and detached pair.

Experiment 9.—Oxygen and deutoxide of nitrogen produced a slight effect upon the iodide; the effect subsided after the circuit had been complete for a few minutes. On examining the battery after the circuit had been closed for twenty-four hours the liquid in the oxygen tubes had not risen; in the tubes containing deutoxide of nitrogen the liquid had risen somewhat unequally in the different tubes to an amount averaging 0·2 cubic inch; in the detached pair it had risen to the same amount. Not the slightest voltaic effect was now produced by the terminal wires.

Experiment 10.—Oxygen and olefiant gas decomposed the iodide, but rather feebly; after the circuit had been closed for twenty-four hours there was still a decomposition, which continued, but the action was extremely feeble. Two cells were allowed to remain arranged in closed circuit for fifteen days, a third being placed by the side, but with the terminals unconnected; at the expiration of this time the rise of liquid in the tubes was as follows:—

Rise of liquid in cells of closed circuit, in tubes of

Oxygen . . .	0·05 cubic inch.
Olefiant gas . .	0·4 „

Rise of liquid in cells of detached pair, in tubes of

Oxygen . . .	0·02 cubic inch.
Olefiant gas . .	0·3 „

Rise of liquid apparently due to voltaic action,

In oxygen tubes .	0·03 cubic inch.
In olefiant gas tubes .	0·1 „

These quantities are too small to enable any satisfactory inference to be deduced as to the equivalents of these gases which contributed to electrolysis; the more so as the rise of liquid was not quite uniform, and the action due to solution was so much greater than that due to electrolysis.

I do not feel entitled to draw any other conclusion from this experiment than that there was a very feeble voltaic current produced by these gases; both the remaining oxygen and the olefiant gas were unaltered in character.

Experiment 11.—Oxygen and carbonic oxide produced notable effects upon the iodide, and slight symptoms of decomposing water; a few bubbles gathered upon the electrodes of an interposed voltameter; the effects continued; and at the expiration of fifteen days the following was the state of the tubes in two cells, put aside as in the last experiment:—

Rise of liquid in cells of closed circuit,

In oxygen tubes . .	0·12 cubic inch.
In carbonic oxide tubes .	0·93 „

Rise of liquid in tubes of detached pair,

In oxygen tubes . .	0·02 cubic inch.
In carbonic oxide tubes .	0·7 „

Rise of liquid apparently due to voltaic action,

In oxygen tubes . .	0·1 cubic inch.
In carbonic oxide tubes .	0·23 „

Before the battery was charged for this experiment the carbonic oxide had been carefully freed from carbonic acid by caustic potash. After action the liquid gave a slight precipitate with lime-water, showing that carbonic acid had been produced by the action. In this experiment the rise was more uniform in the different tubes than in the last, and the action more decided. The results, although on a small scale, appear more definite; thus we get the proportion as 1 : 2·3; and as the combining volumes of oxygen and carbonic oxide are as one to two, if we add the local action due to the oxygen of the air in solution, 1 to 2·3 is as near an approximation as can be expected. Though much superior to olefiant gas, the action of carbonic oxide is, however, very feeble when compared with that of hydrogen.

Experiment 13.—Oxygen and chlorine. Very considerable action on the iodide at first, but not constant; it abated within the first hour, and after twenty-four hours the action was extremely feeble, scarcely perceptible: the water had risen nearly to the top of the chlorine tubes, but the level in the oxygen tubes was unaltered. The chlorine was negative to oxygen, or, in other words, the oxygen was in its voltaic bearing to chlorine as hydrogen to oxygen.

As in this experiment the water-level in the oxygen tubes was unaltered, it appeared that this gas had little to do with the action; I therefore, in

Experiment 14, charged the alternate tubes of a battery with chlorine and dilute sulphuric acid; the amount of action was much the same as in experiment 13, and equally transitory; a few gaseous bubbles were perceptible on the platinums in the oxygen cells, but not in sufficient quantity for examination. It is well known that chlorine of itself will slightly decompose water, forming hydrochloric acid, and evolving oxygen, and there is little doubt that the voltaic action here observed was due to this. There was no appearance of the platinum having been attacked in several experiments which I made with chlorine. So slight a chemical action will, however, give rise to voltaic effects, that the absence of any apparent corrosion is not conclusive. It is stated by chemists that gaseous chlorine will not attack platinum, but that it is only when nascent it combines with this metal; *non constat*, however, that in the gas battery the chlorine at the initiatory instant of its electro-synthesis may not be in a state analogous, as to its chemical energies, to that converse state called nascent, and therefore we cannot venture to negative the possibility of the platinum being slightly attacked. This circumstance, added to its extreme solubility and power of decomposing water, makes chlorine rather an unsatisfactory element for the class of actions developed by the gas battery.

Solutions of bromine, chlorine, and iodine have been before experimented on (I believe by Dr. Schœnbein and M. Becquerel) as to their voltaic relations, but in examining the voltaic relations of bodies in a gaseous state, or, to express myself

with more caution, in a state passing from gaseous to liquid, I tried

Experiment 15.—One set of tubes charged with gaseous chlorine, and the alternate tubes with solutions of bromine and iodine. The chlorine was negative to both, *i.e.* was to these as oxygen to hydrogen.

I now tried hydrogen with several gases; but as it was next to impossible (I found it quite impossible), in experiments on a large scale, perfectly to exclude atmospheric air from the solution,* voltaic action was produced in every case; and as, with one exception (chlorine), oxygen was the most powerful electro-negative gas, the action of the atmospheric air entirely masked any effect which might have been produced by the other gases.† I shall, therefore, not go through these experiments in detail, but mention one or two only which appear interesting, for the reasons which I shall state.

Experiment 16.—Chlorine and hydrogen gave very powerful effects, as was expected by Dr. Schœnbein;‡ water was decomposed between platinum electrodes by two cells. This is the most powerful gas battery, but not very satisfactory, for the reasons above stated (Experiment 13).

Chlorine, in its voltaic relations, may be considered as the converse of zinc, both decomposing water, but the one liberating oxygen, the other hydrogen; thus, a tube of the gas battery charged with chlorine, and having acidulated water as an electrolyte, and zinc as a positive element, forms a combination of which one pair will decompose water. I have tried to render this combination practically useful, by charging the negative cell of a diaphragm battery with peroxide of manganese and muriatic acid, but the supply of chlorine thus obtained is insufficient for quantitative voltaic effects, though the intensity is great.

Experiment 17.—Hydrogen and carbonic oxide were tried

* Gases will creep by a species of endosmose through water. Some time ago I kept inverted over water for two months a vessel divided by a diaphragm of porous ware, on one side of which was oxygen gas, on the other hydrogen; the diaphragm was constantly wet from capillary attraction; at the end of that period the water had risen considerably, and the gases on each side detonated.

† See Postscript.

‡ See his letter, *Phil. Mag.*, March 1843.

in order to ascertain their voltaic relations. Hydrogen was much more electro-positive than carbonic oxide, or rather formed, with the oxygen of the atmospheric air in solution, a combination which overpowered the opposite tendency of the carbonic oxide and air.

Experiment 18.—Chlorine and olefiant gas gave a very feeble effect upon iodide of potassium. After four hours the liquid in the olefiant gas tubes had not risen more in the closed circuit than in the detached pair; the chlorine was nearly all absorbed in solution.

Experiment 19.—Chlorine and carbonic oxide gave very notable effects; ten cells decomposed water. From the extreme solubility of the former gas the equivalent relationship could not be ascertained.

It now occurred to me that as oxygen and hydrogen are evolved from water by electrolysis, and conversely form water by electro-synthesis, so some other gases which are evolved from certain electrolytes by voltaic action might, when arranged as a gas battery with the electrolyte from which they are evolved, give rise to a current, although they would not do so when arranged in circuit with a different electrolyte. To test this view I tried

Experiment 20.—Oxygen and deutoxide of nitrogen, in alternate tubes of the gas battery, with dilute nitric acid; the effects were, however, precisely similar to Experiment 9, viz. a very feeble action for a few minutes, then a cessation, and no continuous chemical action.

Experiment 21.—For the same reason oxygen and nitrogen, with solution of sulphate of ammonia, were tried; this arrangement produced at first a slight effect upon the iodide, which soon ceased, and after several days there was no more rise of liquid in any cell of the closed circuit than in the detached cell; the rise of liquid in both was very trifling indeed (about 0·01 cubic inch), and had evidently nothing to do with voltaic action. In this experiment, and in every experiment that I have tried, I have perceived a trifling action for the first few minutes. This I should have attributed to accidental causes, such as slight impurities in the gases, slight metallic deposits on the plates, &c., but that it is always in the direction which

the theory of the gas battery would indicate. Thus, in the present experiment, the appearance of iodine indicated oxygen to have the same voltaic relation to nitrogen as it has to hydrogen. This temporary effect, therefore, appears to me analogous to that action called by Continental experimentalists polarisation, an apparent tendency to action, *i.e.* an arrangement of molecules preliminary to electrolysis, but incapable of producing a continuous current. In this and many other experiments with the gas battery I have observed this effect, but have never been able to produce any chemical change or electro-synthetic absorption of nitrogen.

Experiment 22.—As oxalic acid when electrolysed evolves at the anode a mixture of oxygen and carbonic acid, and at the cathode hydrogen and carbonic oxide, for the reasons above stated I charged a gas battery with carbonic acid and carbonic oxide in the alternate tubes, and with oxalic acid as an electrolyte; a slight effect was produced, the carbonic oxide being to the carbonic acid as hydrogen to oxygen; but the current was evidently due to the atmospheric air in solution combining with the carbonic oxide; this I proved by some of the test experiments before-mentioned, which I need not recapitulate.

Experiment 23.—Hydrogen, nitrogen, and sulphate of ammonia. This combination also gave effects with which the nitrogen appeared to have nothing to do, this gas being perfectly unaffected. I tried other experiments on this point, but they all led to the same conclusion, viz. that my idea of realising a voltaic action by conversion of the ordinary effects of electrolysis was erroneous. It may be that the above gaseous products of electrolysis are secondary, and that water is the only electrolyte in these cases; but for this, as for many other theoretical questions, there are so many arguments *pro* and *con*, that it is not worth while to dilate on them unless they can be shown to lead, or to be likely to lead, to some new valuable facts or natural relations.

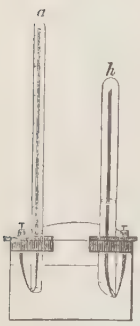
Reviewing the above experiments, it appears that chlorine and oxygen, on the one hand, and hydrogen and carbonic oxide, on the other, are the only gases which were decidedly capable of electro-synthetically combining so as to produce a

voltaic current.* I should perhaps except olefiant gas, which appears to give rise to a continuous though extremely feeble current; and the vapours of bromine and iodine, were they less soluble, would probably also be found efficient as electro-negative gases.

It now occurred to me that as several of these gases (take as an instance nitrogen) were absolutely without effect in the gas battery, this would form a valuable instrument for the analysis of atmospheric air or other mixed gases. I therefore procured,

Experiment 24.—Two narrow cubic inch tubes of seven inches long, carefully graduated into 100 parts. These were immersed in separate vessels of dilute sulphuric acid, and filled with atmospheric air exactly to the extreme graduation; the water-mark within the tube was examined when exactly at the same level as the exterior surface of the liquid; folds of paper were used to protect them from the warmth of the hands, and thus prevent expansion; the barometer and thermometer were examined, and every precaution taken for accurate admeasurement. One of these tubes was left empty, in order to ascertain, and eliminate from the result, the effect of solubility. Into the other was placed a strip of platinised platinum foil, one quarter of an inch wide. This strip of foil was connected by a platinum wire with another strip placed in a tube of hydrogen and inserted in the same vessel. The apparatus is shown in fig. 12. After the circuit had been closed for two days, the liquid was found to have risen in the tube *a* twenty-two parts out of the 100; in the tube placed by its side it had risen one division. The tubes were allowed to remain several days longer, but no farther alteration took place. This analysis gives, therefore, twenty-one parts in 100 as the amount of oxygen in a given portion of air.

Fig. 12.



Experiment 25.—The tube *a* (fig. 12) was charged with nitrogen to a given mark, and 0·5 cubic inch of pure hydrogen added; the tube *h* was then charged with oxygen, and the circuit closed. Examined after twenty-four hours, the water had risen in the tube *a*

* See Postscript.

exactly 0·5 cubic inch. The apparatus was left in this state for several days, but without any farther effect; the voltaic action had thus perfectly exhausted the hydrogen and there stopped.

These experiments are sufficient to prove the accurate eudiometric action of the gas battery. Performed on a large scale this method of eudiometry appears to me likely to possess some advantages. In the eudiometer of Volta, when gases containing oxygen are to be analysed, if the hydrogen added for detonation be impure the result is of course erroneous. The same may be said of the detonation by spongy platinum, or by a wire heated by a voltaic current, which I formerly proposed.*

If, on the other hand, gases containing hydrogen are to be analysed, errors may result from any impurities in the oxygen which is added, or from inaccuracy in the measurement of either gas; in the electrolytic method of eudiometry the quantity or purity of the hydrogen, in the one case, is of no importance; and in the other the quantity or purity of the oxygen; that is, provided there be sufficient to exhaust the equivalent to be abstracted from the mixed gas subjected to examination.

It should be observed, that in these experiments only a single pair of the gas battery can be used, as, if more be employed, the electrolyte is likely to be decomposed, and gas added to the compound.† The process is rather slow, but I think very sure. Another valuable application of this process is, that it affords (in Experiment 24) a simple method of obtaining nitrogen of unquestionable purity. I know of no method which effects this object so perfectly. All the oxygen of the air is abstracted, as well as that free oxygen which may be contained in the liquid; and by subsequently introducing a little lime-water into the tube *a*, the trifling quantity of carbonic acid may be removed, or the same thing may be at once effected by using caustic potash as the electrolyte in the apparatus, fig. 12.

Probably many other applications of the gas battery may suggest themselves to other experimentalists, and obviously

* *Phil. Mag.*, August 1841, p. 99.

† See Postscript.

many more changes may be rung upon the gases employed, and curious and valuable results obtained; I have, however, in this paper given a sufficient number of experiments fairly to open the subject; each appears so suggestive of new ones, that it is difficult to know where to stop.

The experiments on eudiometry, which I have last named, induced me to refer to Dr. Henry's paper on Gaseous Analysis,* and on reading it I was struck with a coincidence between the action of spongy platinum on mixed gases and the gas battery, a coincidence strongly confirmatory of the views which led me to its discovery. I will endeavour briefly to state these, and I state them, not as being absolutely correct, for differences of opinion may exist on this as on every other scientific matter, but as being those which existed in my mind prior to the experiments, and which are considerably, and to me unexpectedly, strengthened by the results embodied in the above-mentioned paper of Dr. Henry.

My original deduction may be stated and exemplified as follows:—When pure or amalgamated zinc is immersed in acidulated water the oxygen, as is well known, will not combine with the zinc; but touch both zinc and liquid with platinum and combination ensues, the platinum being unaltered. So, with a mixture of oxygen and hydrogen, the gases, although in intimate contact, will not chemically unite; but touch them with clean platinum and more or less rapid combination ensues. Here also the platinum is unaltered. Leaving out of the case any purely hypothetical explanation, why may not effects so similar in their character be related in other respects? In the voltaic combination the platinum is heated during action; and if the surfaces, and consequently the quantity of electro-chemical action, be considerable, it is ignited; so in the catalytic combination, if the platinum be thin and of large extent, or in the form of a sponge, which still more increases its surface, it is ignited. Why, therefore, may we not regard the detonation of gas by platinum as a voltaic effect? or the combination of oxygen and zinc by the presence of platinum a catalytic effect? The only difference is, that gases do not admit of that interchangeable relation of particles which we call electrolysis. The necessity for this

* *Phil. Trans.*, 1824.

interchange is, however, removed when the gases are in a state of such intimate admixture that it is not requisite to convey the action through a chain of particles; in the gas battery this chain is supplied by the intervening electrolyte, and thus the same action which is local in the experiments of Döberciner is circulating in the gas battery. The latter bears the same relation to the former as the action of the ordinary voltaic battery does to the normal phenomena of chemical affinity. This relation is confirmed by the facts detailed in the paper of Dr. Henry, as the gases which he there found would combine by the presence of spongy platinum are precisely those which will combine in the gas battery; thus oxygen and hydrogen combine rapidly, oxygen and carbonic oxide much more slowly, and oxygen and olefiant gas very feebly, so much so, as, in Henry's experiments, to require heat to induce combination. Of course chlorine and hydrogen, which will unite without platinum, will, *à fortiori*, unite with the aid of platinum, or they may in the gas battery occasion secondary action; the oxygen evolved by the decomposition of water by the chlorine combining with the free hydrogen in the tube. As oxygen and ammonia will, when at a slightly elevated temperature, combine by the influence of spongy platinum, forming water and leaving nitrogen, I now, in order farther to test this relation, tried

Experiment 26.—Ten cells of the gas battery were charged with oxygen and solution of ammonia, with a little sulphate of ammonia added to improve its conducting power. This arrangement produced a moderate effect upon the iodide, which was continuous; the liquid rose slowly but uniformly in the oxygen tubes; a gas was evolved in the alternate tubes, which proved to be pure nitrogen. After three weeks closed circuit, the gases, collected, measured, and averaged, gave for each tube—

Nitrogen evolved	= 0·07 cubic inch.
Oxygen absorbed . . .	= 0·12 cubic inch.

Experiment 27.—To examine whether the mere alkaline character of ammonia had anything to do with the effect, ten cells were charged with oxygen and solution of caustic potash, but produced no effect.

These experiments are strongly corroborative, and seem to me conclusive as to the relation between the action of the gas battery and catalysis by spongy platinum. Experiment 26 is also remarkable in regard to the binary theory of electrolysis, but upon this point I will not here enter.

Applying the hypothesis of Grotthus to the gas battery, we may suppose that when the circuit is completed, at each point of contact of oxygen, water, and platinum, in the oxygen tube, a molecule of hydrogen leaves its associated molecule of oxygen to unite with one of the free gas; the oxygen thus thrown off unites with the hydrogen of the adjoining molecule of water; and so on until the last molecule of oxygen unites with a molecule of the free hydrogen; or we may conversely assume that the action commences in the hydrogen tube. In these hypotheses we should bear in mind that we proceed by steps which nature, as hitherto tested by experiment, has not recognised. All we can safely predicate of the actions at anode and cathode is that they are correlations; although they take place at a distance, the one has no more been proved to take place without the other, or before the other, than height has been proved to exist without depth. I therefore allude to this hypothesis, not as explicitly adhering to it, but because it is generally received, and may tend to associate the action of the gas battery with the ordinary phenomena of electrolysis.

A number of hypotheses has been and may be proposed to account for these and other mysterious phenomenal relations; they all agree in being assimilations of what is unfamiliar to what is familiar. They are undoubtedly useful as illustrations, and it is as such that they have hithero contributed to advance science. It is, however, a curious circumstance, and worthy of some consideration, that the voltaic hypothesis of Grotthus, the emissive and undulatory hypotheses of light and heat, and, as far as I am aware, all physical hypotheses hitherto propounded, represent natural agencies as effects of *motion* and *matter*. These two seem the most distinct, if not the only conceptions of the mind, with regard to natural phenomena; and when we try to comprehend or explain affections of matter which are not obviously modes of

motion, we hypothetically or theoretically reduce them to it: the senses *perceive* the different effects of sound, light, heat, electricity, &c., but the mind appears capable of distinctly *conceiving* them only as modes of motion. Does not this supply an argument that all physical agencies are reducible to these elements of mental conception? Or are we to look for new powers of mind, in other words, will greater familiarity with phenomena, at present recondite, enable the mind more clearly to comprehend them, and avoid the necessity of referring them theoretically to more familiar and apparently more simple phenomena? To pursue this curious enquiry would involve me in a discussion foreign to the object of this paper and to the general character of contributions to the Royal Society, but the question arises so immediately out of the subject, and is so necessary to explain my own view, that I trust this brief statement of it will be considered sufficiently pertinent. It touches upon that interesting, scarce definable boundary, where physical merges into metaphysical science.

There are one or two other theoretical points as to which the gas battery offers ground of interesting speculation; the contact theory is one. If my notion of that theory be correct, I am at a loss to know how the action of this battery will be found consistent with it. If, indeed, the contact theory assume contact as the efficient cause of voltaic action, but admit that this can only be circulated by chemical action, I see little difference, save in the mere hypothetical expression, between the contact and chemical theories; any conclusion which would flow from the one would likewise be deducible from the other; there is no observed sequence of time in the phenomena: the contact or completion of the circuit and the electrolytical action are synchronous. If this be the view of contact theorists, the rival theories are mere disputes about terms. If, however, the contact theory connects with the term contact an idea of force which does or may produce a voltaic current independently of chemical action, a force without consumption, I cannot but regard it as inconsistent with the whole tenor of voltaic facts and with general experience.

Another point of theory suggested by the gas battery is the relation of latent heat in the different cells of the battery

and voltameter. According to our received theory of caloric, oxygen and hydrogen cannot assume the gaseous from the liquid state without rendering sensible heat latent. Now, as in the gas battery the gases evolved from the liquid in the voltameter must require and absorb precisely as much heat as is set free by the gases becoming liquid in each cell, it may be a curious subject of future enquiry (an enquiry which that beautiful instrument, the thermo-multiplier, will materially aid) to ascertain whether the heat absorbed in the voltameter be exacted from surrounding bodies, or whether it be supplied by the action of the battery itself, *i.e.* as the chemical force in the voltameter is conversely equivalent to that in each cell of the battery, and the calorific force at the voltameter is also the converse equivalent of that in each battery cell, whether there is the same mutual dependence of the latter as of the former forces. The action in the voltameter of ordinary batteries would argue strongly against the proposition, that the heat is exacted from surrounding bodies, as it is well known that water when electrolysed has its temperature rather increased than diminished; and I have found, when decomposing water with the nitric acid battery at a rate of 150 cubic inches a minute, a very considerable augmentation of temperature in the liquid subjected to decomposition, so much so, that, if the quantity was not considerable, it was heated to ebullition. Much of this adventitious heat may have arisen from the restriction of the circuit by the voltameter plates and connecting wires; but if the gas battery be supposed to supply exactly sufficient heat, or (to use a license of expression) to convert electricity into sufficient heat to satisfy the demands of the expanding gases—each battery cell being able by the condensation of its respective gases to afford this supply—a rise of temperature ought to be perceptible in the whole battery equal to the heat produced by the condensation of gases in all the cells, minus that of one cell. I have not as yet been able to detect any elevation of temperature due to the action of the gas battery, not having in my possession any instrument capable of detecting such delicate thermoscopic effects. I am, therefore, the more anxious to offer the point for the consideration of those who may have such instruments at their

command, and here for the present I leave the gas battery and its theory.

Postscript, July 7.—The length of time which has elapsed between the communication and printing of this paper, as it has enabled me to procure the apparatus fig. 8, will, I trust, be deemed a sufficient reason for my adding a postscript containing a few experiments with this form of battery, some of which I cannot but consider important.

Experiment 28.—In order farther to test the opinion expressed, p. 274, six cells of this battery were charged with pure hydrogen and dilute acid in the alternate tubes. When first charged they decomposed water freely, but after the circuit had been closed for a short time, to exhaust the oxygen of the atmospheric air in solution, they produced no voltaic effect; the whole series of six would not decompose iodide of potassium; when, however, a little air was allowed to enter any one of the tubes containing liquid that single cell instantly decomposed the iodide; three cells were put aside, each in closed circuit; at the expiration of a week these produced no effect upon a galvanometer, nor was there any gas evolved in the tubes containing liquid; the stoppers were now taken out, and the liquid in the hydrogen tubes rose to an average of 0·3 cubic inch; each cell contained a pint, and we may therefore regard 0·15 cubic inch as the amount of oxygen held in solution by this quantity of acidulated water. Were it not for the extreme practical difficulty of perfectly excluding atmospheric air for a long period, the above would furnish an excellent method of examining the quantity of oxygen held in solution by water, and by applying the proper calculus we might read off on our galvanometer scale the infinitesimal bubbles of gas contained in a given bulk of liquid; if, however, the acid water or the hydrogen contain foreign ingredients, a very different result follows, and the liquid, for reasons which will now be obvious, frequently rises considerably in the hydrogen tubes.

Experiment 29.—I repeated experiment 24 with the battery fig. 8, expecting that as the external air was shut out I should obtain the result more speedily; I was indeed not without a vague hope of producing some effect upon the nitrogen. The

first result did follow: upon taking out the stoppers the morning after the battery had been charged, the liquid rose in the air-tube one-fifth of the gaseous volume. I now closed it again and examined it three days afterwards; a very curious effect had taken place; the volume of the gas in the air-tube, which had previously contracted, had now *increased*, and it continued slowly increasing day after day. I at first believed that the nitrogen was decomposed, but after many conjectures and experiments found that the increase was due to the addition of hydrogen. On repeating the experiment with nitrogen instead of air the same effect took place, but of course without the previous contraction. I now returned to battery fig. 4; several of these cells charged, some with atmospheric air and hydrogen, and others with nitrogen and hydrogen, did not exhibit the effect, though suffered to remain six weeks, each in closed circuit.

To ascertain whether the vacuum formed by the abstraction of oxygen from the liquid had anything to do with the above effect, a central narrow tube, open at both ends, was substituted for the stopper in the battery fig. 8; the hydrogen was still evolved. Not to detail a tedious set of test experiments, I at length found that two points were essential to obtaining the effect with certainty; first, the exclusion of any notable quantity of atmospheric air from solution; and secondly, great purity in the hydrogen. In the former case, when the hydrogen could find oxygen to combine with, it was not evolved; in the latter, there would be mixed or rather diluted gas on both sides, and the forces would be balanced; thus I have never succeeded in obtaining the effect in the open battery, fig. 4 with hydrogen obtained in the ordinary way from granulated zinc or iron filings, but have sometimes succeeded with hydrogen procured by electrolysis. In the battery, fig. 8, I have succeeded in producing the effect, but in a feeble degree, from hydrogen obtained in the common way, but have never failed with hydrogen obtained by electrolysis. Oxygen of the greatest purity, voltaically associated with nitrogen, does not produce a similar effect. The above unexpected results render it necessary, in order to ensure accuracy in the eudiometric

Experiment 24, either purposely to use common hydrogen in the batteries figs. 4 and 12, or, what is more expeditious and accurate, to use a battery similar to fig. 8, but with tubes longer in proportion to their width; and having first charged the tubes with hydrogen and atmospheric air, to allow these to remain in closed circuit until all the oxygen is abstracted and a little hydrogen added, by the electrolytic effect, to the residual nitrogen; then to substitute oxygen for the original hydrogen, which will in its turn abstract the hydrogen from the nitrogen and leave only pure nitrogen. I have frequently done this with perfect success.

Experiment 30.—Hydrogen and carbonic acid in battery fig. 8 produced the same effect. The volume of the carbonic acid was increased, and hydrogen was found to have been added to it. The effect therefore is not due to any peculiarity of nitrogen, but yet some gas is necessary, for Experiment 28 proves that hydrogen alone will not decompose water. I need scarcely say that when the above-mentioned effect took place an interposed galvanometer was deflected, but the current was much too feeble to decompose iodide of potassium.

I have tried, associated with hydrogen in battery fig. 8, carbonic oxide, olefiant gas, protoxide of nitrogen, and deutoxide of nitrogen; the two former produced no current or chemical effect, the two latter gave a current and were decomposed. The volume of the deutoxide contracted one-half; the residual gas was found to be nitrogen, which thenceforth was gradually increased by hydrogen. The volume of the protoxide did not undergo the previous contraction, except slightly from solubility, but its change of state was denoted by the absorption of hydrogen in the associated tube.

I likewise tried the effect of a vacuum and hydrogen, by charging a battery (fig. 8) with 1 cubic inch oxygen and 3 cubic inches hydrogen; the current was much enfeebled by the resistance offered by the vacuum; at first iodide of potassium was decomposed and the galvanometer needle whirled round; after twenty-four hours the galvanometer needle was only deflected 10°; thus a physical was opposed to, and resisted, a chemical force; the current, however, continued, and all the gas in the oxygen tube disappeared, except a minute

bubble; this was probably nitrogen from the atmospheric air in solution, which had escaped to fill the vacuum. When the stopper was taken out the liquid rose suddenly in the hydrogen tube 2·2 cubic inches, giving the equivalent of the oxygen in the tube and in solution. It is very possible that this experiment repeated might sometimes exhibit an evolution of hydrogen in the oxygen tube arising from the escape of the nitrogen of the atmospheric air in solution, and acting as in Experiment 29, but I have not seen this effect take place. It should be distinctly understood that in all the experiments mentioned in this postscript, except the first part of Experiment 28, *single* cells only were used.

Upon the theory of the Experiments 29 and 30 I will venture no positive opinion. That gaseous hydrogen should *abstract oxygen from hydrogen*, without the latter forming any other combination, is a fact so novel, that any attempted explanation is likely to prove premature. If, contrary to the views of Dalton, we suppose that gases when mixed are held together by a feeble chemical affinity, then we may say that the affinity of the nitrogen or carbonic acid for hydrogen produces the effect; the affinity of the oxygen of the water, being balanced between the hydrogen in the liquid and that in the tube, would enable the resultant feeble affinity of the nitrogen for hydrogen to prevail; but on this supposition why does not oxygen produce an analogous effect? Its tendency directly to combine with platinum may indeed be regarded as an opposing force, but this tendency is by many considered hypothetical. On the other hand, it may be called an effect of contact; but this, unconnected with a chemical theory, presents no other idea to the mind than the fact itself presents—it furnishes no link by which we may extend the phenomena. I therefore, until a better theory be found, should be inclined to adopt the former view, and to regard mixed gas as in a state of feeble chemical union, the more especially as throughout nature we find no absolute lines of demarcation, though for conventional reasons we are obliged to adopt them. There must be many cases in which it is difficult, if not impossible, to draw the line between mechanical mixture and chemical combination.

In conclusion, I would say, with regard to the whole of the experiments contained in this paper, that a longer time and more experience may give positive results in cases where I have only obtained negative ones ; it is far from impossible that since curious solid combinations are formed by slow electrical currents, as in the experiments of Crosse and Becquerel, so novel gaseous or liquid products may be obtained by the long-continued voltaic action of gases and liquids. This time alone can show.

For previous experiments and theories on the combination of gases by platinum I may refer to Döbereiner's paper, 'Phil. Mag.,' Oct. 1823, in which I find he expresses an opinion that it is a voltaic effect ; to the papers of Dulong and Thenard, 'Annales de Chimie,' tom. 23 and 24 ; and to Faraday's 'Experimental Researches,' series 6. The various experiments on polarised electrodes of Ritter, Faraday, De la Rive, Becquerel, Matteucci, and Schœnbein are also in point.

VOLTAIC ACTION OF PHOSPHORUS, SULPHUR, AND HYDROCARBONS.

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In a paper which was honoured by publication in the 'Philosophical Transactions' for 1843 I described certain forms of the gas voltaic battery, together with a series of experiments in which different gases were employed as voltaic combinations, and the consequent application of voltaism to eudiometry.

To ensure confidence in the accuracy of the eudiometric experiments it was essential that the position which I laid down as to the absence of all voltaic action in a combination of oxygen and nitrogen should be rigidly true. I may state with certainty that it is so, but an apparent exception noticed (Experiment 21) in my last paper obtains during the first few

minutes after the circuit is closed, and sometimes for a much longer time. The examination of this temporary action in the first instance, with the view of ascertaining whether it was a specific action of the nitrogen or attributable to adventitious circumstances, led me to the results which I have the honour of laying before the Royal Society in this paper.

Before detailing these results I will, for convenience sake, premise that they were all obtained by the form of battery represented in fig. 8 of my last paper (which, with a slight addition, to be referred to presently, is again represented at fig. 2), p. 294, charged with distilled water slightly acidulated with pure sulphuric acid.

I will also, when alluding to my last paper, to avoid the needless repetition of the word *experiment*, refer to the number of the experiments as though they were paragraphs, and continue those numbers in the paragraphs of this paper.

As the form of battery (fig. 2) by which the interfering action of the atmosphere is entirely prevented was not devised until the greater part of the experiments in my last paper had been completed, I repeated some of them which seemed to require such verification with this battery. To one of these only is it essential that I should now refer. I should likwise mention that in the experiments to be described the proper reductions for temperature and pressure have been made when necessary; where it was practicable the experiments were examined on days when the temperature and pressure were, as nearly as may be, the same as when they were set by.

(31.) Oxygen and deutoxide of nitrogen, which in the open form of battery gave only a temporary action (9), when employed in the closed form (fig. 2) gave a continuous current. The following three sets of experiments were continued each for a month in closed circuit, during which time they were constantly tested by the galvanometer, and evidenced a continuous voltaic action; at the expiration of the month the results were as follows:—

Experiment 1. Rise of liquid in tubes of — Cubic in.

Oxygen . . . . . =0·32

Deutoxide of nitrogen . . =1·26

		Cubic in.
Experiment 2.	In oxygen tubes	=0·5
	Deutoxide of nitrogen . .	=2·5
Experiment 3.	In oxygen tubes	=0·2
	Deutoxide of nitrogen . .	=0·75
Mean result	In oxygen tubes, rise . . .	=0·34
	In deutoxide tubes, rise . .	=1·5

The slight excess being undoubtedly due to the greater solubility of the deutoxide, it appears that four volumes of deutoxide of nitrogen are absorbed in the gas battery for one of the associated oxygen, and the result would be a compound of 1 equiv. nitrogen + 3 oxygen, or hyponitrous acid, which is exactly that formed by the slow combination of these two gases in the ordinary chemical way. The difference of amount of action in the three experiments depended on the temperature, the second experiment being made towards the close of last summer, the last experiment during the continuous cold weather of the present spring.

These experiments, coupled with the converse ones with hydrogen and deutoxide of nitrogen (30), afford very satisfactory instances of the illustration of the law of definite combining volumes by the gas battery, exhibiting in one result, and itself registering that result, the action of equivalent chemical combination, catalysis and voltaism.

(32.) I now pass to the experiments which will form the more immediate subject of this paper. The temporary action to which I have alluded (21) being greater when nitrogen and oxygen were the gases used, if the nitrogen were obtained by burning phosphorus in atmospheric air, than if procured from other sources, it naturally occurred to me that this action was due either to some phosphorous acid remaining in a state of vapour in the nitrogen, or to a slight portion of the phosphorus itself being held in solution in the nitrogen, as believed by Vauquelin and the older experimentalists. If this last supposition were the correct one, it seemed to offer a means of rendering phosphorus, though a non-conductor and insoluble in aqueous liquids, yet a permanent voltaic excitant analogous to the oxidable metals.

(33.) A small piece of phosphorus having been carefully

dried, and weighing when dry 9·6 grains, was passed up through the liquid into the large tube of a gas battery by means of a small loop of mica, which kept it separated both from the glass and the platinum; the tube was now charged with pure nitrogen, and the associated tube with pure oxygen, the level of the gases or water-mark being noted by a little slip of paper pasted on the tube; a check experiment of oxygen and nitrogen without phosphorus was charged at the same time; the whole was carefully closed from the atmosphere and set by for twenty-four hours in closed circuit, to get rid of any current from adventitious circumstances; the next day, when tested by the galvanometer and iodide of potassium, a very decided action was apparent in the phosphorus battery, the iodide being decomposed and the galvanometer needle swinging round to 30°, the nitrogen with the phosphorus representing the zinc of an ordinary voltaic combination; the check experiment gave not the least deflection or decomposition. The experiments were suffered to remain in closed circuit for four months, from August 10 to December 14, 1844, having been frequently tested in the interim, and the galvanometer always evidencing a continuous voltaic action in the phosphorus battery. On the 14th of December the water in the oxygen tube having by its rise denoted the absorption of a cubic inch of oxygen, plus the slight quantity 0·05 cubic inch of oxygen due to solution, as proved by comparison with the second battery, the experiment was examined: the result was as follows:—

Rise of liquid in oxygen tube, 1 cubic inch.
„ „ in nitrogen tube, 0.
Original weight of phosphorus, 9·6 grains.
Present weight of phosphorus, 9·2 grains.

The battery was again charged in a similar manner, and put by on December 19, 1844; the phosphorus weighed 2·8 grains. This, in consequence of the extremely cold weather which has prevailed almost without intermission from that time to the present period, proceeded much more slowly, and was not examined until May 17, 1845, when the results were as follows:—

Permanent deflection of galvanometer, 8°.

Rise of liquid in oxygen tube, 0·35 cubic inch.

„ „ in nitrogen tube, 0.

Weight of phosphorus = 2·65.

Taking a mean of these two experiments, which in their relative results approximate more closely than I could have anticipated under the circumstances, we get 0·415 as the proportional weight of phosphorus lost for a cubic inch of oxygen. Now, as 24 : 31·4 :: 0·34 : 0·444. The result of these experiments therefore leaves no doubt that phosphorous acid is the product of the voltaic action, as it is of the slow combustion of phosphorus in air. The experiment was repeated with distilled water; the action was at first very trifling, but increased every day, and the water gradually acquired an acid reaction.

No light was apparent in any part of the apparatus when examined in the dark; indeed, the action was much too slow to render such an effect probable; though if subsequently by heat or other means I should succeed, as I hope, in producing light, it will be curious to observe in what part of the circuit the luminous effect in the voltaic combustion is perceptible.

A series of ten cells of phosphorus and nitrogen associated with oxygen were charged, and perceptibly decomposed water with platinum electrodes.

The result of the above experiments gives, I believe, the first instance of the employment of a solid, insoluble non-conductor as the excitant of a continuous voltaic current; it proves that the existence of diffused phosphorus in nitrogen, as noticed by the old experimentalists, is not a consequence, as was once believed, of a partial combustion, but of an effusion continuing as long as the previously diffused phosphorus is abstracted, and it gives the very curious result of a true combustion, the combustible and the '*comburant*' being at a distance; phosphorus burned by oxygen which is separated from it by strata both of water and gas, of an indefinite length. This result, arrived at by a progressive series of inductions, scarcely now appears extraordinary, but would have been in all probability listened to with incredulity if simply stated as a fact a few years ago. By the galvanometer we may also

ascertain the rate of this very slow and minute chemical action; thus, if by an apparatus as above described, my galvanometer gives a deflection of 8 degrees, I know by the above results that the phosphorus is being consumed at the rate of the seven millionth part of a grain per minute.

(34.) The next step was to ascertain whether this action was peculiar to nitrogen or common to other gases; for this purpose, a day or two after the first experiment was set aside, the following were also made, and the dates and results were as follows:—

No. 1. Phosphorus suspended in protoxide of nitrogen associated with oxygen: weight of phosphorus 5·3 grains. Charged August 11, 1844.

No. 2. Similar experiment, but without phosphorus.

Tested occasionally by galvanometer, the first battery gave invariably a small deflection, but less than in experiment (33); the second gave no deflection.

Examined April 22, 1845.

No. 1. Water risen in tube of oxygen 0·75 cubic inch.

In protoxide tube, 1·7 inch.

No. 2. Water risen in oxygen tube 0·1 cubic inch.

In protoxide tube, 1·6 cubic inch.

Phosphorus weighed 5 grains, therefore loss = 0·3 grain.

In this experiment the rise of liquid in the tubes containing protoxide was evidently due to the solubility of that gas, as it was very nearly equal in both the batteries, and the second gave not the slightest galvanometric deflection; the result gives 0·65 cubic inch of oxygen consumed by 0·3 grain of phosphorus, bearing nearly the same relative proportions as experiment (33); the only difference between the action of phosphorus in nitrogen and in protoxide of nitrogen is, that in the former it is more rapid, as proved both by the galvanometric deflection and by the quantity of oxygen absorbed in a given time.

(35.) Charged August 11, 1844.

No. 1. Phosphorus in carbonic acid gas associated with oxygen; weight of phosphorus 5·9 grains.

No. 2. Same, without phosphorus.

Tested by galvanometer, No. 1. always gave a deflection, No. 2. none.

On the 3rd of December the carbonic acid gas in both batteries had been absorbed, and the liquid had reached the extremities of both tubes.

In the oxygen tube of

No. 1, rise of liquid = 0·75 cubic inch.

In No. 2, 0·05 cubic inch.

Phosphorus weighed 5·6 grains; the proportional weight was therefore 0·3 grain phosphorus to 0·7 cubic inch oxygen.

Here again the proportions came out just as in (33) and (34), the intensity of action being intermediate, less than the former and greater than the latter.

(36.) Charged December 18, 1844.

No. 1. Phosphorus in pure oxygen associated with oxygen, great care being taken to exclude atmospheric air: this arrangement having been kept in closed circuit for twenty-four hours, gave a very feeble deflection of the galvanometer.

Examined February 15, 1845. The rise of liquid in the tube containing phosphorus was equal to 0·3 cubic inch, in that containing the associated oxygen = 0·05. I find no note of the phosphorus being re-weighed; probably I considered it useless, as the consumption of oxygen in the associated tube was so very trifling, scarcely sufficient to be distinguished from the effect of its solubility.

(37.) Charged April 23, 1845.

No. 1. Phosphorus suspended in deutoxide of nitrogen associated with oxygen; weight of phosphorus = 4·3 grains.

No. 2. Same, without phosphorus.

Examined May 27, 1845. Galvanometer gave 25° permanent deflection in No. 1, and 10° in No. 2.

No. 1. Rise of liquid in deutoxide tube = 0·7 cubic inch.
Rise of liquid in oxygen tube = 0·6 cubic inch.

No. 2. Rise of liquid in deutoxide tube = 0·7 cubic inch.
In oxygen tube = 0·2 cubic inch.

Weight of phosphorus 4·17 grains.

Consequently had lost 0·13 grain for 0·4 cubic inch of oxygen.

In this and all the preceding experiments the residual gases were unchanged in quality, and in this experiment it appears that the action of the deutoxide of nitrogen and the oxygen was perfectly unaffected by the phosphorus, the con-

sumption of deutoxide of nitrogen being exactly the same in both batteries. In another experiment, which I did not record, on account of a minute bubble of air having entered the tubes containing the deutoxide, the phosphorus appeared to have exercised a retarding influence on the voltaic combination of deutoxide of nitrogen and oxygen; this I attributed to a slight deposit of phosphorous acid upon the platinum, by which its catalytic power was deteriorated.

(38.) It thus appears that the effect we have been examining, of the diffusion of phosphorus in gas, is not due to any peculiarity of nitrogen, and is not peculiar to any particular gas, as once believed; but being in all probability common to all gases which do not exercise a specific action on the phosphorus, it may be more properly called a volatilisation of phosphorus at ordinary temperatures than a solubility in gas; the ordinary slow combustion of phosphorus in air is, in fact, a combustion of its vapour. I incline to think that the inferiority of its vaporisation in pure oxygen is due to a protecting film being formed, and that the phenomenon is in some respects analogous to the inactivity of iron in nitric acid.

(39.) Phosphorus in nitrogen was associated with hydrogen in the gas battery, to ascertain their voltaic relation; the hydrogen was positive to the phosphorus, *i.e.* represented the zinc of an ordinary voltaic combination.

(40.) To realise the curious novelty of two non-conducting solids forming the elements of a voltaic battery, and producing a continuous current, phosphorus suspended in nitrogen in one tube of a gas battery was associated with iodine in nitrogen in the other; a very decided current was the result, which continued for months, the nitrogen remaining unaltered in volume, but the liquid becoming gradually tinged by the excess of iodine vapour. The following is the result of the experiment:—

Charged January 1, 1845.

Weight of iodine . .	=5·9 grains.
Weight of phosphorus .	=6·4 grains.

Examined May 17, 1845.

Weight of iodine . .	=4·6 grains.
Weight of phosphorus .	=6·28 grains.

The phosphorus has consequently lost 0·12 grain, the iodine 1·3. Assuming that the phosphorus consumed 3 equivalents of oxygen, as in experiments (33), (34), (35) (37), we should have 3 equivalents of hydrogen eliminated, and consequently 3 of iodine consumed, or

$$31{\cdot}4 : 126{\cdot}6 :: 0{\cdot}12 : 0{\cdot}48$$
$$0{\cdot}48 \times 3 = 1{\cdot}44.$$

The result is tolerably near, but from the iodine vapour in solution an excess and not a deficit in the consumption of this was to have been expected.

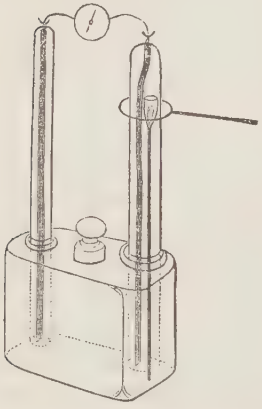
(41.) It was necessary for my own satisfaction to make a great number of other experiments for the purpose of checking and eliminating any adventitious results which might possibly interfere with the actual voltaic action of the gas battery, such as placing phosphorus in single tubes containing the different gases, but with platinum foil and without associated tubes, others without the platinum foil or associated tubes; but as these had no influence on the results, and were merely used as tests for my own satisfaction, it would be useless and tedious to detail them.

(42.) Having examined the action of phosphorus in the gas battery under these various circumstances, my next step was to ascertain if any other substance produced a similar effect. Sulphur, the nearest analogue of phosphorus, was the body which naturally presented itself, but from its different characteristics required a different mode of manipulation. The following was adopted. Into a little capsule of glass, having a long solid leg (see fig. 1), was placed a small piece of solid sulphur; this was held in the large aperture of a gas battery cell, while the tube was passed carefully over it; the platinum in this tube was connected with the zinc of a single cell of the nitric acid battery, while an anode of platinum was placed in the liquid through the central aperture; by this means all the oxygen of the atmospheric air was exhausted, and the surplus hydrogen was in turn taken away by connection with the associated tube charged with oxygen; the same effect might have been more slowly produced by the process

Fig. 1.

described (29). The sulphur was now in an atmosphere of pure nitrogen, and this could have been effected in no other way that I am aware of without wetting or forming some deposit on the sulphur. Having connected it in closed circuit with the oxygen tube for twenty-four hours, the galvanometer gave no deflection. A small hoop of iron with a handle was now heated and passed over the tube containing sulphur and nitrogen, the wires being connected with the galvanometer (see fig. 2). The result was very striking. I had directed my assistant to watch the galvanometer while I attended to the manipulation. At the same instant he exclaimed that the galvanometer was deflected, and I that the sulphur was melting; the galvanometer continued deflected during the whole time that the sulphur remained fused, and indeed some time afterwards, until all the sulphur vapour diffused in the nitrogen had become exhausted. The sulphur represented the zinc of an ordinary voltaic combination. It was of course impracticable in this case to ascertain the equivalent consumption. This experiment very strikingly exhibits the analogy of sulphur with phosphorus, and proves that the instant sulphur is fused it becomes a volatile body, as phosphorus is when solid; the suddenness of its action, coupled with the insoluble character of sulphur, leads to the conclusion that solution in the electrolyte is not a necessary antecedent to voltaic action in the gas battery. Indeed, this might have been deduced from the experiments with phosphorus, as its vapour must have been nearly, if not absolutely insoluble in the electrolyte, or the equivalent results would not have come out so accurately; possibly solution and electrolysis are in these cases synchronous.

Fig. 2.

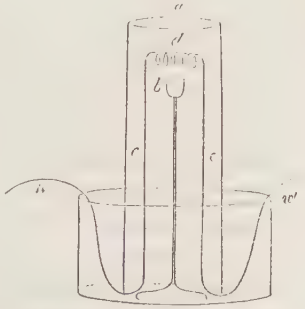


(43.) I was now led to try in the gas battery other substances differing from phosphorus and sulphur, but possessing characters which had hitherto prevented their being used as

voltaic excitants; as, if my view of the volatility of phosphorus and sulphur were correct, other volatile bodies ought to act similarly. Camphor was the first substance I experimented on. A piece of camphor weighing 12·9 grains was placed in a similar manner to the phosphorus (33) in nitrogen, and associated with oxygen; tested by the galvanometer it gave a feeble deflection, which, however, was continuous; it was allowed to remain four months in closed circuit; at the expiration of that time the liquid had risen in the oxygen tube 0·3 cubic inch; the nitrogen in which the camphor was suspended had increased in volume 0·15 of a cubic inch. The camphor weighed 11·4 grains, but some minute crystals of it were observed at the top of the tube, so that the loss of weight was greater than that due to voltaic action.

(44.) The smallness of the quantity of the gas which had been added to the nitrogen precluded an accurate analysis of it; enough was ascertained, however, to lead me to believe that it was hydrocarbonous, and it then became my aim to produce it in greater quantities. I attached a piece of camphor to a platinum wire, and to the same wire I also attached a piece of sponge platinum; I passed these up into a tube of nitrogen over distilled water, and at the expiration of three months the gas had increased 0·1 cubic inch; this proved that the camphor vapour was decomposable by the catalytic action of platinum at ordinary temperatures, and that the effect in the nitrogen cell of the battery was not due to its voltaic association; but the experiment did not give me a sufficient quantity of the gas for analysis.

Fig. 3.

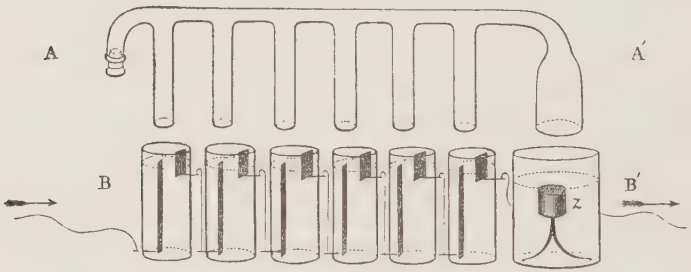


(45.) I therefore had recourse to the apparatus, fig. 3. *a* is an inverted cylindrical test-glass; *b* a platinum capsule with a pin-hole in the bottom for drainage, standing on an ivory pedestal; *c*, *c* two very stout platinum wires; *d* a coil of fine platinum wire. Into the capsule *b* was placed the camphor, the glass *a* filled with distilled water was inverted over

it and charged with pure nitrogen, to a level marked somewhere below the capsule; the wires *w w'* are now connected with a voltaic battery of sufficient power fully to ignite the wire *d*.

(46.) After the wire was ignited the volume of the gas gradually increased; when the original volume was doubled the gas was examined. It had a strong disagreeable odour, very similar to that of coal-gas; it burned with a blue flame, slightly tinged with yellow: placed in an eudiometer, such as I formerly described,* and mixed with hydrogen, it underwent no alteration. Two volumes of it, mixed with one volume of oxygen, contracted one-sixth of the whole volume, and, subsequently agitated with lime-water, contracted two-sixths more, lining the tube with a crust of carbonate of lime. The residual gas was nitrogen. It was thus clear that

Fig. 4.



the vapour of camphor was decomposed by the ignited wire into carburetted hydrogen and carbonic oxide, and the analogy is too direct to leave any doubt that these gases were also formed in Experiments (43) and (44) by the influence of the platinum foil and spongy platinum.

The apparatus (fig. 3) offers a most convenient means of decomposing volatile hydrocarbons, and possibly other substances.

(47.) Portions of oil of turpentine and of cassia were now placed in capsules (fig. 1), weighed and exposed each to an atmosphere of nitrogen in the large tube of a gas battery,

* *Phil. Mag.*, Aug. 1841, p. 99; and *Phil. Trans.*, 1843, p. 105.

by the same means as described (42); they gave a very decided deflection (the nitrogen representing zinc). This deflection continues, the rise of liquid has been slow but continuous, and the galvanometer feebly deflected. In the turpentine experiment the rise is = 0·7 cubic inch, in the cassia 0·5; the weights, however, from the irregularity of absorption and evaporation, give no data as to the equivalent consumption; thus, the turpentine has lost 0·7 grain, the cassia gained 0·05 grain.

(48.) Alcohol and ether were tried in a similar manner, and produced notable voltaic effects; alcohol the most powerful probably, on account of its greater solubility in water.

(49.) The *rationale* of the action in Experiments (43) and (47) is curious. It seems that the platinum in the nitrogen tube first decomposes the vapour of the hydrocarbons,* and then the same platinum, with its associated plate, re-combines the separated constituents with oxygen. In Experiment (43) the decomposition takes place more quickly than the re-composition, as indeed would be expected from the absence of the resistance of the electrolyte in the former case, and hence the increase of gas in the nitrogen tube.

(50.) The analogy of the action of the above volatile substances strengthens the position advanced (38), that solid phosphorus should be regarded as volatile at ordinary temperatures, and sulphur when fused; the whole of these experiments also serve to introduce the galvanometer as a new and delicate test, and in some cases a measurer of volatilisation.

(51.) As the gas battery was shown in the former paper, which I had the honour to communicate to the Society, to give us the power of introducing gases which had been previously untried as voltaic excitants, and to ascertain their electro-chemical relations, it has, by the means detailed in this paper, opened a field for ascertaining the voltaic relations and quantitative electro-chemical combinations of solid and liquid substances, which from their physical characteristics had not hitherto been recognised in lists of the voltaic relations of

* I use this word here and in the title to avoid periphrasis; it is not quite correct as applied to some of these bodies.

different substances, and consequently formed to a certain extent a blank in the chemical theory of the voltaic pile. These results, coupled with the previously-arranged tables of electro-chemical relations, and with the great improvements in apparatus for measuring these relations recently made by Mr. Wheatstone and others, offer every promise of the ultimate establishment of accurate measures of affinity. I give the following tables as an approximative list, without attempting to give the degrees of intensity, which can only be filled up by a careful series of researches exclusively devoted to this object :—

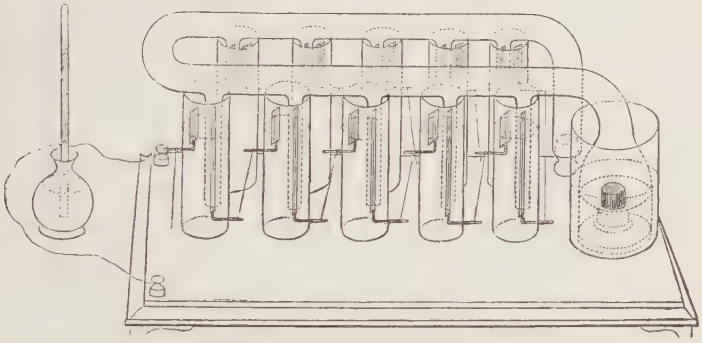
(52.) Chlorine.	Camphor.
Bromine.	Essential oils.
Iodine.	Olefiant gas.
Peroxides.	Ether.
Oxygen.	Alcohol.
Deutoxide of nitrogen.	Sulphur.
	Phosphorus.
Carbonic acid.	Carbonic oxide.
Nitrogen.	Hydrogen.
Metals which do not decompose water under ordinary circumstances.	Metals which decompose water.

Though carbonic acid and nitrogen appear to be neutral, and consequently might be bracketed with the metals which do not decompose water, as forming the nodal point or zero of the table, yet, in consequence of the peculiar action exercised by them, and detailed (29) and (30), I have placed them above the metals.

(53.) The results embodied in my present and my former paper I think sufficiently indicate the field of research opened by the gas battery, a field which may of course be indefinitely extended. I have never thought of the gas battery as a practical means of generating voltaic power, though in consequence of my earlier researches, which terminated in the nitric acid battery, having had this object in view, I have been deemed by some to have proposed the gas battery for the same

purpose; there is, however, a form of gas battery which I may here describe, which, where continuous intensity or electromotive force is required, but the quantity of electricity is altogether unimportant, appears to me to offer some advantages over any form of battery hitherto constructed, and which, independently of any practical result, is, from circumstances peculiar to the gas battery, not without interest. It is shown at figs. 4 and 5. A A′ is a long glass tube, with a series of legs or glass tubes attached to and opening into it; the lower extremities of these are open, and the main tube or channel

Fig. 5.



A A′ terminates at the extremity A in a glass stopper, and at A′ opens out into a funnel, as shown in the figure. Into a series of glasses B B′ are cemented platinum wires having attached to them strips of platinised platinum foil, two to each glass, the one being four inches long and half an inch wide, the other $1\frac{1}{2}$ inch long by one inch wide; the former set are placed lower than the latter, so that when the glasses are filled with liquid the former set shall be just covered, and the latter bisected by the water-mark; the last glass B′ has no platinum. These platinum strips are connected metallically by external wires, the narrow platinum of one cell with the wide one of the next, and so on in series. The glasses having been filled to the top of the narrow platinum with acidulated water, let a piece of zinc be placed on a pedestal in the vessel B′, and the stopper being out of the extremity A, the apparatus A A′ lowered into the glasses, the tubular legs covering each one of

the narrow platinum plates. The tubes will of course be full of water, and the main channel full of atmospheric air; this will gradually be displaced by the hydrogen ascending from the zinc, which hydrogen, in consequence of the curve at A, will retain its position. When it is judged that the greater portion of air has been expelled, the stopper at A, covered with a little grease, is to be inserted; the hydrogen now will rapidly descend in all the tubes until the zinc is laid bare, and then remain stationary.

We have now a gas battery the terminal wires of which will give the usual voltaic effects, the atmospheric air supplying an inexhaustible source of oxygen, and the hydrogen being renewed as required by the liquid rising to touch the zinc; by supplying a fresh piece of zinc when necessary, it thus becomes a self-charging battery, which will give a continuous current; no new plates are ever needed, the electrolyte is never saturated, and requires no renewal except the trifling loss from evaporation, which indeed is lessened, if the battery be in action, by the newly-composed water. There is an aperture in the pedestal with a moveable slide, through which the vessel B′ can be removed, when necessary, to replace the zinc, and the remaining part of the apparatus is never disturbed. This battery would form an elegant substitute for the water battery; it would much exceed in intensity a similar number of series of that apparatus; it would be applicable to experiments of slow crystallisation, and possibly to the telegraph. Its construction is difficult, and makes its prime cost expensive, but after that it is the most durable, the most easily charged, and the most free from local action of any known form. I have had one of ten cells constructed, shown at fig. 5 which succeeds perfectly, giving sparks, decomposing water, &c., and is ever ready for use. Any number of such sets might be united by adapter-tubes, or indeed it would be much more economical, and reduce to a minimum the damage from breakage, to have the main channels A A′ made of varnished wood or porcelain, with apertures into which separate glass tubes might be cemented.

THERMOGRAPHY AND VOLTAISM.

Lecture at the London Institution.

Literary Gazette, Jan. 21, 1843.

THE following facts Mr. Grove had himself observed with regard to metallic thermographs. 1. When two portions of the same metals are juxtaposed each being at the same temperature little or no effect is produced. 2. When at a different temperature a slight effect is perceptible. 3. When the metals are different at the same apparent temperature a greater effect is perceptible. 4. When the metals are different, and at different temperatures, a still greater effect is visible. Again, taking, for example, a sovereign placed on a silver or copper plate, the effect is increased if the coin be breathed on before being brought into contact; it is still more increased if the coin be rubbed with oil and wiped apparently dry; and it is still increased if it be held for an instant over the vapour of a substance capable of chemically acting on the juxtaposed plate; as, for instance, over ammonia, before being placed on copper. These facts led him to believe that such impressions were caused by a radiation and condensation of whatever vapour exists between the metals, and which, by being condensed unequally (in the case of different metals, by their different conducting and radiating powers), produce an impression according to the unequal distance of the different parts of the stamp on the coin. In fact, if we rightly understood him, the phenomenon was in some degree analogous to dew. Be this as it may, whatever the nature of the radiation be, there is unquestionably some radiation; and this fact of inter-radiation was applied by Mr. Grove to explain a fundamental experiment in voltaic electricity, hitherto a subject of much controversy, being the effects of Volta's original experiment on the contact and severance of two discs of dissimilar metals. The contact theory of electricity attributes the effects on the electroscope produced by this instrument to simple contact; the

antagonistic theory asserts that no electrical disturbance can take place without some chemical or physical change. Now, as the facts of thermography prove a radiation of some sort (for this argument, no matter of what sort) to take place on the juxtaposition of different metals, a physical change is produced, electricity ought to be developed on their separation, caused either by their action on each other, or by the different degrees of evaporation from each of their surfaces when separated; this physical or chemical change ought to produce, and does produce, a disturbance of electric equilibrium; and thus recourse to the incomprehensible action of simple contact is unnecessary. An experiment was shown strongly confirmatory of this view: the discs of zinc and copper were juxtaposed, surface to surface, but contact prevented by a rim of paper, and yet when separated the electroscope was affected; here, then, was *no metallic contact*, and yet the electrical effects were produced, which should be so by the mutual radiation, but which should not be so by the contact theory.*

VOLTAIC PHENOMENA.

Electrical Mag., Sept. 1843.

In the 'Philosophical Magazine,' for October 1839, I have noticed the fact that when copper formed the anode of a voltaic pile in nitro-sulphuric acid, it became inactive, and was neither oxidated, nor was oxygen evolved from its surface; differing in the latter respect from inactive iron, it perfectly arrested electrolytic action, and consequently the passage of the voltaic current. Some facts which I subsequently observed will throw some light on this anomaly, and, as far as I am aware, are in themselves new.

* See Mr. Gassiot's confirmation of this, avoiding the rim of paper. *Phil. Mag.*, Oct. 1844.

If a copper wire form the anode of a voltaic battery of moderate power in dilute sulphuric acid, it is, as is well known, gradually oxidated and dissolved. If the intensity of the battery be increased, I have found that, at a certain point, (varying with the voltaic combination employed), the action is arrested, the copper is no longer dissolved, and a galvanometer interposed in the circuit gives little or no indication of a current. Let the intensity be now farther increased, by gradually adding to the voltaic series, and a very curious phenomenon is exhibited; the copper is apparently disintegrated, boiling off, as it were, in a very fine reddish-brown powder; the heat is intense, and the electrode rapidly worn away; the powder thus formed is partially dissolved, provided the acid be not already saturated; after saturation it subsides, and may be collected and examined; it proved to be the second oxide, or copper 32 + oxygen 8, called by some authors the protoxide, by others the deutoxide. Besides employing the usual chemical tests, I analysed it voltaically, by comparing the weights lost from the anode with the hydrogen evolved at the cathode; the following are three experiments taken from my note-book, dated April 27, 1841 :—

Thermometer, 60° Fahrenheit.

Exp. 1. For 6 cub. in. of hydr. collected, anode lost 4·2
„ 2. „ „ „ 4·2
„ 3. „ „ „ 4·3
3)12·7
4·23

Now, 47·2 : 32 :: 6 : 4·07.

This result was obtained with a common pair of scales, and although too coarse for a very accurate determination of the equivalent, was quite sufficient to prove that it was the full oxide, which was all I required.

The battery employed varied from 1 to 20 cells of the nitric acid combination. I generally found that the intensity at which the current was arrested was when from 5 to 10 cells were employed—12 to 16 seldom failed to give the disintegrating effect.

The points worthy of notice in this experiment are—

1st.—The purely mechanical effect of the voltaic current, the oxide being forcibly detached without any farther chemical combination.

2nd.—The neutral point when the current is arrested, the intensity being too great for the usual action of solution, and too feeble for the disintegrating action.

3rd.—The intense heat generated, which here, as in all other voltaic experiments, appears to be proportionate to the resistance.

When, with a very powerful voltaic battery, small wire electrodes of platinum are dipped into water acidulated with sulphuric acid, a brilliant combustion, attended with a decrepitating noise, is observed at the cathode, provided the anode be sufficiently immersed in the liquid; on inverting the arrangement, *i.e.* immersing the cathode and touching the surface with the anode, a slight spark only is perceptible; in both cases the heat is very intense, and the liquid boils at the point of contact, giving off sulphurous acid fumes; it would seem, at first, that this phenomenon was contrary to the usual fact observed in the voltaic combinations, viz. that the combustion is more active at the positive than at the negative terminal; but I found it to be in reality no anomaly, as it is the sulphur of the dilute acid which is principally concerned in producing the brilliant effect at the cathode, and which, as it is evolved, forms itself the positive terminal. When the experiment was continued for some time a very dark chocolate powder was deposited in the liquid, and was found to be precipitated when ingredients of the utmost purity were employed. After various tests had been employed, this precipitate was ultimately found to be soluble in nitro-muriatic acid; the solution, treated with ammonia, gave a double salt of platinum, it also gave a precipitate with chloride of barium; the original deposit was, in short, sulphuret of platinum. The gas evolved from the negative electrode in this experiment was hydrogen, mixed with sulphuretted hydrogen.

Entertaining a strong opinion that electricity is *not* a specific fluid or fluids, but an affection of matter (possibly a mode of motion); and, believing that all electrical phenomena are intermolecular changes of the bodies, through which what

is called the current of electricity passes, the following question occurred to me in some experiments on the calorific effects of voltaic electricity. If the ignition of a thin wire be the result of a change in the arrangement of its own particles, caused by an antagonist force to that which determines its aggregation, what will be the effect of this force, pushed to the extreme?—*e.g.* Voltaically ignite a given wire to fusion, arrange it so that after fusion the wire shall retain its position in the voltaic circuit, and what will be the result? The experiment was tried with platinum and lead wires, placed in a little gutter of unglazed porcelain, which would retain the metal after fusion. The effect upon the platinum wire was as follows: after fusion the wire appeared to swell slightly, and then burst asunder with a snapping noise; one of the frustra again ignited, again burst, and so on, until the portions became too thick to be fused by the voltaic force employed. The broken ends presented no very remarkable appearance. My deduction from this experiment was, that the voltaic current tended to expand the wire in the transverse, and to contract it in the longitudinal direction.

With wires of lead the effects were more remarkable. After fusion this metal evinced a tendency to contract in length; and, the direction of this contraction being followed by gently approaching the terminal wires, which touched its extremities, it appeared, as it were, to germinate; it shot out an irregular series of nodules. I may rudely compare the effect to a membranous gullet, tied with a series of ligatures, and then distended to the utmost. Each nodule pressed upon its neighbour and formed dividing facets, which, when cold, could be easily separated by the nail or by a blunt penknife.

The experiment, when carried on for some time, generally ended either in the breaking asunder of the lead (though, from its want of elasticity, unattended with the snapping noise), or else the circuit was interrupted by the oxidation of the lead.

THE BAKERIAN LECTURE. — ON CERTAIN PHENOMENA OF VOLTAIC IGNITION AND THE DECOMPOSITION OF WATER INTO ITS CONSTITUENT GASES BY HEAT.

Phil. Trans., R.S. *Received* September 3. *Read* November 19, 1846.

In the 'Philosophical Magazine' for August 1841, I recommended for eudiometrical purposes the use of a platinum wire ignited by a voltaic battery. In fig. 1, is represented a form of apparatus for this purpose; it consists of a tube of Bohemian glass, with a loop of platinum wire $\frac{1}{80}$th of an inch diameter sealed into its upper end; the size of the glass tube may be adapted to the quantity of gas sought to be analysed, and may, when necessary, be reduced to extremely small dimensions, one-eighth of an inch being ample; into this the gas may readily be made to ascend, by the insertion of a wire of copper, platinum, or glass, as may be suitable to the gas; two cells of the nitric-acid battery are sufficient fully to ignite the wire, and the same battery supplies, by electrolysis, pure oxygen and hydrogen for the analysis. Since the period when I first proposed this I have seldom used any other apparatus for such gaseous analyses as are performed by combining the gas to be examined with oxygen or hydrogen. This eudiometer possesses the advantage of enabling the operator either to detonate or slowly to combine the gases, by using different powers of battery, by interposing resisting wires, or by manipulation alone—a practised hand being able, by changing the intervals of contact, to combine or detonate the gas at will. My general practice has been to produce a gentle heat in the wire until the gases contract, and then gradually to increase the heat until a full ignition takes place, by which means all the objects of the eudiometer of Volta are fulfilled, without detonation, without dependence on the fickle electric spark, and without thick tubes, danger of explosion, or of the gases being projected from the eudiometer.

I have commenced with a description of this eudiometer,

as it has been indirectly the means of my undertaking the experiments detailed in this lecture; and as its very great convenience has never been generally understood, I think that in strongly recommending it I shall be of service to chemists.

In a paper honoured by insertion in the 'Philosophical Transactions' for 1845, page 358, I have shown another method of eudiometry also performed by voltaic ignition; in that experiment the vapour of camphor was decomposed into carbonic oxide and carburetted hydrogen; it was an application of voltaic ignition to effects analogous to those produced by Priestley and others, by passing compound gases through ignited tubes of porcelain.

But the voltaic process has this immense advantage, that the heat can be rendered incomparably more intense; that the quantity of vapour or gas to be operated on may be indefinitely small; that there are no joints, stop-cocks or ligatures; and that there is no chance of endosmose, which takes place through all porcelain vessels. I therefore determined to examine by these means several gases, both with a view of verifying, under different circumstances, known results, and seeking for new effects by this new and advantageous application. I used an eudiometer (fig. 1) of 8 inches long and 0·4 inch internal diameter, exposing the gases to intense heat, and subsequently analysed the residues in one of the same length, but 0·2 inch diameter.

Fig. 1.

I will first consider the physical effects of different gases on the ignition of the wire itself.

In a paper on the 'Application of Voltaic Ignition to lighting Mines,'* I have mentioned the striking effects of hydrogen in reducing the intensity of ignition of a platinum wire, so much so that a wire voltaically ignited to incandescence in atmospheric air, is apparently extinguished by inverting over it a jar of hydrogen; with other gases the effects are not so striking, and with them these differences are

* *Phil. Mag.*, Dec. 1845.

best shown by including a voltameter in the circuit. Davy found that the conducting power of a wire diminished in proportion to the degree to which it was heated: assuming the accuracy of this position, the amount of gas in the voltameter would be inverse to the intensity of ignition in the wire. The following is the result I obtained with different gases, employing the same battery (the nitric acid combination at its most constant period), the same wire, and the same vessel:—

Gases surrounding the wire.	Cubic inches of Gas evolved in the voltameter, per minute.
Hydrogen	7·7
Olefiant gas	7·0
Carbonic oxide	6·6
Carbonic acid	6·6
Oxygen	6·5
Compressed air, 2 atmospheres	6·5
Nitrogen	6·4
Atmospheric air	6·4
Rarefied air	6·3
Chlorine	6·1

To ascertain the relation between the amount of radiant heat generated by the same battery and wire in gases which presented striking differences as to the luminous effects of the platinum wire, an apparatus was prepared in which the bulb of a thermometer was retained at a certain distance from the coil of wire ignited by a battery of four cells, and exposed, first, to an atmosphere of hydrogen, and then to one of atmospheric air, at the same temperature and pressure; the thermometer rose $7\frac{1}{2}^{\circ}$ in five minutes in the hydrogen, and 15° in the air in the same time. Both the heating and luminous effects appear therefore to be greater in atmospheric air than in hydrogen. I cannot satisfactorily account for the differences shown in the above table; there appears a general tendency to greater ignition in the electro-negative than in the combustible gases, but the facts are far too few to found a generalisation. I was at first inclined to regard the difference of effect in hydrogen as analogous to the peculiarity mentioned

by Leslie * respecting its convection of sound, but the parallel does not hold: sound is transmitted imperfectly through rarefied air, and also through hydrogen; on the contrary, the heat of the ignited wire is most intense in the former, and least so in the latter; the heat is also very much reduced in intensity in the compounds of hydrogen, ammonia and olefiant gas, or even by a small admixture of hydrogen with another gas, such as nitrogen; hydrogen, therefore, appears to have a peculiar and specific action in this respect.

I now pass to the consideration of the effects of the ignited wire on different gases. The ignition was in every case raised to the fullest extent, and the gases after exposure to it were carefully cooled down to their original temperature.

When the experiments were made over water, the whole

Fig. 2.

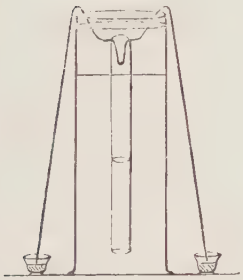
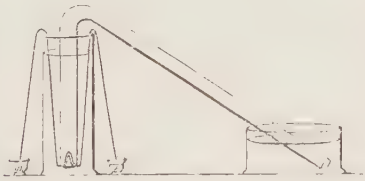


Fig. 3.



eudiometer was immersed in a vessel of distilled water, occasionally having an inch depth of oil on the surface (see fig. 2†); when they were made over mercury, and a long-continued exposure was required, a bent tube was employed, as at fig. 3, the closed end being immersed in water or oil, to prevent the fusion of the glass, which would otherwise have ensued.

The tubes are much more easily preserved from cracking, and the ignition better kept up with oil on the exterior than with water; but, as in many of these experiments I might have been considerably misled by a crack in the glass, or a

* *Trans. of the Cambridge Phil. Soc.*, vol. i. p. 267.

† In this and in figs. 3 and 5 the lines leading from the platinum loop to the mercury cups represent copper wires.

bad sealing of the wire, allowing a portion of oil to enter the tube, I used water in the greater number of them until I was assured of the phenomena.

The apparatus, fig. 3, is superior in one respect to fig. 2, even for experiments over water, as the wire being situate below the volume of gas, the circulation is more rapid.

Fig. 4.

This object may also be effected by employing the form of eudiometer, fig. 4, in which the loop of wire is near the centre of the tube, so as to be just above the surface of water in the tube; there are, however, some difficulties of manipulation with this form, which render it practically of less value than fig. 1.

Binoxide of nitrogen over distilled water contracted differently in proportion to the heat of the wire; in the best experiment it contracted to one-third of its original volume; the residual gas was nitrogen. Nitric acid was found in solution in the water.

Over mercury the effects were nearly the same: the mercury was attacked, and the orange fumes of nitrous acid were visible.

Protoxide of nitrogen was decomposed into nitrogen and oxygen; the volume increased by 0·35 of the original volume; I could not get the full equivalent proportion, or 0·5 of oxygen.

Carbonic acid underwent no perceptible alteration.

Ammonia increased to double its original volume; it was now no longer absorbable by water, and gave 3 volumes of hydrogen, plus 1 nitrogen.

Olefiant gas contracted slightly, deposited carbon, the residue being hydrogen and olefiant gas, more of the former in proportion to the heat, but I could not succeed in entirely decomposing it.

Nitrogen suffered no change.

Oxygen gave a very slight contraction, amounting to $\frac{1}{50}$th of its volume; the oxygen employed was very pure, obtained from chlorate of potash and manganese, and also from water by electrolysis: no change in properties was perceptible in the oxygen after its exposure to the ignited wire. This contraction I incline to attribute to a slight portion of hydrogen present, which view will, I think, be considered as strengthened by the effect of the ignited wire on hydrogen, to be presently

detailed. I at one time thought that the contraction might be due to a slight oxidation of the wire, but it never went beyond a very limited point; nor was the wire altered in size or weight, though it was kept ignited for many hours.

Chlorine over water gave dense white fumes; a greyish yellow insoluble powder accumulated on the sides of the tube near the platinum wire, which appeared of the same nature as the vapours; the deposit was insoluble in cold nitric, sulphuric, or muriatic acid, but dissolved by the last when boiled. The fumes did not, as far as I could judge, affect litmus-paper; a barely perceptible tinge of red was indeed communicated to it, but this, I had every reason to believe, was attributable to a slight portion of muriatic acid not absorbed by the water. I have not yet worked out this result, as it is probable, considering the number of experiments that have been made on heated chlorine, that it is a known product, though I cannot find, in several books to which I have referred, any substance answering to it in description, and the field opened by voltaic ignition is so new that each result demands a separate and prolonged examination; if I find that this is an unknown compound I shall probably resume its investigation.*

Cyanogen gave, though in very minute quantities, a somewhat similar deposit, but at its then very high temperature it began to act rapidly on the mercury, and I was obliged to give up the experiment after an hour's ignition. Both these gases require peculiar and novel apparatus for examination by voltaic ignition. It will presently be seen that my whole attention and disposable time were necessarily occupied with certain phenomena to which this class of experiments ultimately led me.

Hydrogen gave a very notable contraction, amounting in some cases to one-tenth of its volume. This was an unexpected result, and I examined it with care. It took place both over water and over mercury; rather more with the former than with the latter. It obtained equally with hydrogen procured by electrolysis from carefully distilled water and pure

* See Supplemental Paper, p. 20.

sulphuric acid; with that procured from common zinc and pure sulphuric acid diluted with distilled water; and with that obtained from distilled zinc and pure diluted sulphuric acid. The contraction was less when the water from which the hydrogen was obtained was carefully purged of air by boiling and the air-pump, but yet there was a notable contraction even when the water had been freed from air to the utmost practicable extent. In the numerous experiments which I made on this subject the contraction varied from the $\frac{1}{10}$th to the $\frac{1}{30}$th of the whole volume.

After many fruitless experiments I traced it to a small quantity of oxygen which I found hydrogen to contain under all circumstances in which I examined it. Phosphorus placed in hydrogen, obtained with the utmost care, gives fumes of phosphorous acid, shines in the dark and produces a slight contraction, but there is after this a farther contraction by the use of the ignited wire.

I may cite the following as an easy experiment and simple illustration of the rapidity with which hydrogen appropriates oxygen. Let hydrogen be collected over water well purged of air; let a piece of phosphorus remain in it until all combustion has ceased, the hydrogen will then be full of phosphoric vapour; fill another tube with water, and pass the hydrogen rapidly into it, the second tube will instantly be filled with a dense white fume of phosphorous acid; the hydrogen having instantly carried with it oxygen from the stratum of water it has passed.

A very careful experiment was made as follows:—Distilled water was boiled for several hours, to this was added one-fortieth part by measure of pure sulphuric acid, and it was cooled under the receiver of an air-pump; it was now placed in two test-glasses, connected by a narrow inverted tube, full of the same liquid: platinum electrodes were placed in each glass, and the hydrogen caused to ascend immediately into the eudiometer tubes; the whole was completed within two or three minutes after the water had been removed from the air-pump. Here the ordinary sources of impurity in hydrogen were avoided; no zinc was used, the sulphuric acid was pure, and the quantity was so small, that, had it not been pure, the

error could have been but very trifling. The hydrogen so obtained contracted in volume $\frac{1}{2}\frac{1}{0}$th; hydrogen prepared in the same way, and exposed to phosphorus, gave dense white fumes; the phosphorus was luminous in the dark for more than an hour, and the contraction (temperature and pressure being carefully examined) was $\frac{1}{9}\frac{1}{0}$th; the amount of contraction by the wire would of course equal three times the volume of oxygen mixed with the hydrogen, consequently the oxygen would be $\frac{1}{7}\frac{1}{0}$th of the whole volume; the platinum wire induces therefore a greater absorption of oxygen than the phosphorus, unless the volume of hydrogen is increased by the phosphoric vapour; the sequel of this paper will render it probable that even the ignited wire does not, and cannot induce combination of all the oxygen existing in the hydrogen.

I have looked into the papers of MM. Berzelius and Dulong, and of M. Dumas, on the equivalent weight of hydrogen. The latter contains a most careful experimental investigation, and is by far the best determination we have; although it is not there mentioned that hydrogen contains oxygen, yet a correction is made for the air contained in the sulphuric acid employed. M. Dumas does not state how the quantity of that air is calculated. There can be no question that nothing approaching in elaborate care to these experiments has been yet performed on the subject; but, with the fullest consciousness of M. Dumas' skill, I have, in all my experiments, perceived such an inveterate tendency of hydrogen to possess itself of oxygen, that I cannot help entertaining some doubts whether we have yet the real weight of hydrogen within the assigned limits of error.

It is difficult to see how hydrogen can be absolutely deprived of oxygen which has once existed in it; neither an oxidable metal, as zinc, or an ignited inoxidable metal, as platinum, getting rid of all the oxygen, and phosphorus, if it does so, replaces it by its own vapour. The near approach, however, of the equivalent of hydrogen, as determined by M. Dumas, to the ratio of whole numbers, renders it probable that it is a very close approximation to the truth.

I have not been able to detect nitrogen in the hydrogen,

but the probability is that a slight quantity also exists in it. Whether the oxygen proceeds from portions of air still remaining in solution in the liquid from which the air is exhausted, or whether it is a part of the water actually decomposed, but of which the oxygen is not absorbed by the zinc, is a question to resolve which farther experiments are necessary.

Hydrogen and carbonic acid mixed in equal volumes were readily acted on by the ignited wire; they contracted to 0·48 of the original volume; the residue was carbonic oxide; one equivalent of oxygen had therefore united with the hydrogen; and the slight additional contraction was probably due to the farther combination of hydrogen with oxygen as above stated.

Carbonic oxide exhibited a remarkable effect, and one which, coupled with the last experiment, gave rise to considerations which mainly led to the results to be detailed in the body of this paper. Carbonic oxide, very pure and carefully freed from carbonic acid, was exposed to the ignited wire over distilled water; the gas increased in volume in one experiment to one-third of its original volume, in the greater number of instances to one-fifth: this increase depended upon the intensity of ignition, which it was very difficult to maintain at its maximum on account of the frequent fusions of the platinum wires.

Here again I had a long research and many erroneous guesses, which I need not detail. The effect did not take place with perfectly dry gas over mercury, and I thence was led to attribute it to some combination with aqueous vapour; the increase turned out to be occasioned by the formation of carbonic acid. By agitation with caustic potash or lime-water the gas was reduced to exactly its original bulk, but it was now found to be mixed with a volume of hydrogen equal to the volume of carbonic acid by which it had been increased; it was thus perfectly clear that half a volume or one equivalent of oxygen derived from the vapour of the water, had combined with one volume or equivalent of carbonic oxide, and formed one volume or equivalent of carbonic acid, leaving in place of the carbonic oxide with which it had combined the one

volume or equivalent of hydrogen with which it had been originally associated.

Comparing the last experiment, viz. that of mixed carbonic acid and hydrogen, with this, I was naturally struck with the curious reversal of affinities under circumstances so nearly similar; in the one case, hydrogen taking oxygen from carbonic acid to form water and leaving carbonic oxide; in the other, carbonic oxide taking oxygen from water to form carbonic acid and leaving hydrogen.

I thought much upon this experiment; it appeared to me ultimately that the ignited platinum had no specific effect in producing either composition or decomposition of water, but that it simply rendered the chemical equilibrium unstable, and that the gases then restored themselves to a stable equilibrium according to the circumstances in which they were placed with regard to surrounding affinities; that if the state of mixed oxygen and hydrogen gas were, at a certain temperature, more stable than that of water, ignited platinum would decompose water as it does ammonia.

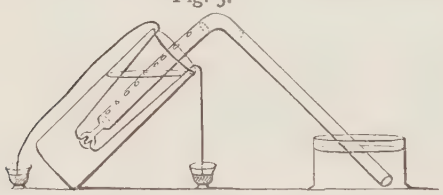
This is a very crude expression of my ideas, but we have no language for such anticipatory notions, and I must adapt existing terms as well as I am able.

It now appeared to me that it was possible to effect the decomposition of water by ignited platinum; that, supposing the atmosphere of steam in the immediate vicinity of ignited platinum were decomposed, or the affinities of its constituents loosened, if there were any means of suddenly removing this atmosphere I might get the mixed gases; or secondly, if, as appeared by the last two experiments, quantity had any influence, that it might be possible so to divide the mixed gases by a quantity of a neutral ingredient as to obtain them by subsequent separation (or as it were filtration) from the neutral substance. Both these ideas were realised.

To effect the first object, after, as usual in such circumstances, much groping in the dark, I cemented a loop of platinum wire in the end of a tube retort similar to fig. 3, and covered it with asbestos, ramming this down so as to form a plug at the closed extremity of the tube, the platinum wire being in the centre. My object was, by igniting the platinum

wire, to drain the water out of the asbestos, and the ignited wire being then in an atmosphere of steam, I hoped the water would by capillary attraction keep constantly oozing down to the platinum wire as the steam or decomposed water ascended. The experiment did not succeed: the water established a current through the asbestos by washing away fine particles, and the phenomena of ordinary ebullition took place, unless the intensity of the battery was very much exalted, when a very slight decomposition was perceptible, which I attributed to electrolysis. This experiment, however, suggested another, which did succeed. In one or two cases the asbestos plug became compressed above the platinum and so choked up the tube that the wire suddenly fused. It now occurred to me that by narrowing the glass tube above the platinum wire I had the result at my command, as the narrow neck might be made of any diameter and length, so as just to allow the water to drip or run down as the steam forced its way up; a tube was so formed, and is shown with its accompaniments at fig. 5.

Fig. 5.



The result of this experiment was very striking: when two cells of the nitric acid battery were applied the air was first expanded and expelled, the water then soon boiled, and at a certain period the wire became ignited in the steam. At this instant a tremulous motion was perceptible, and separate bubbles of permanent gas of the size of pin-heads ascended and formed a volume in the bend of the tube. It was not a continuous discharge of gas, as in electrolysis, but appeared to be a series of rapid jerks; the water, returning through the narrow neck, formed a natural valve which cut off by an intermitting action portions of the atmosphere surrounding the wire: the experiment presented a novel and indescribably curious effect. The gas was oxyhydrogen. It will occur at the first to many of those who hear this paper read that this effect might be derived from electrolysis. No one seeing it would think so for a moment; and although I shall by my subse-

quent experiments, I trust, abundantly negative this supposition, yet as this was my first successful experiment on this subject, and is *per se* an interesting and striking method of showing the phenomenon of decomposition by heat, I will mention a few points to prove that the phenomenon could not be occasioned by electrolysis.

To account for it by electrolysis, it must be supposed that the wire offered such a resistance to the current that this divided itself, and the excess of voltaic power passed by the small portion of water which trickled down, instead of by the wire.

In the first place, the experiment was performed with distilled water, and only two cells of the battery employed, which will not perceptibly decompose distilled water.

2. No decomposition took place until the instant of ignition of the wire, though there was a greater surface of boiling water exposed to the wire before than after the period of ignition.

3. A similar experiment was made, but with the wire divided in the centre so as to form two electrodes, and the water boiled by a spirit-lamp; here the current had no wire to conduct any part of it away, but the whole was obliged to pass across the liquid, and yet no decomposition took place, or if there were any it was microscopic.

Fig. 6.

4. When, instead of oil, distilled water was used in the outer vessel,* even the copper wires, one of which would form an oxidable anode, gave no decomposition across the boiling water outside, while the ignited wire inside was freely yielding mixed gases.

5. To prevent the water from being the shortest line for the current, I repeated the experiment with a perfectly straight wire (fig. 6). The result was precisely the same; but the experiment is more difficult, as a certain length of wire is neces-

* Jan. 8.—I have since found that the exterior tube of oil or water may be dispensed with in this experiment, as the water which trickles down prevents the fusion of the glass.

sary, the sealing is more troublesome, and the size of the bulb is much more difficult to adapt to the production of steam in exactly the requisite quantity: the straight wire being more suddenly extinguished and more easily fused: with careful manipulation, however, it succeeds equally well with the former experiment.

I might add other experiments and arguments, but I believe when the remainder of this paper has been read, that the above will be thought scarcely necessary.

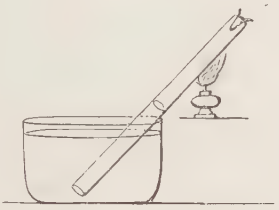
I now directed all my efforts to produce the effects by heat alone without the battery. I will mention a few of my unsuccessful attempts, as it will save trouble to future experimenters. I sealed a platinum wire into the extremity of a curved tube, filled the latter with water, and applied a strong heat by the blowpipe to the projecting end of the wire, hoping that the conducting power of the platinum, although inferior to that of most other metals, was sufficiently superior to that of glass to enable me to ignite the portion of the wire within the tube, and thus surround it with an atmosphere of steam; the water, however, all boiled off from the glass; nor could I succeed in igniting the platinum by heat from without. A similar failure occurred when, on account of its superior conducting power, a gold wire was substituted for that of platinum.

I sealed spongy platinum and bundles of platinum wire into the ends of Bohemian glass tubes, closing the glass over them, and then filling the tubes with water and heating the whole extremity; but the water boiled off from the glass, and the platinum could not be made to attain a full incandescence.

After many similar trials I returned to the battery, and sought to apply it in a manner in which electrolysis could not possibly take place. I had hoped, as I have above stated, to obtain a residual decomposition of water by masking or diluting the gases by a neutral substance. I therefore tried the following experiment: a tube similar to fig. 1 was filled with water which had been carefully freed from air by long boiling and the air-pump; it was then inverted in a vessel of the same water, and a spirit-lamp applied to its closed extremity, until the upper half was filled with vapour (see fig. 7). The wire

was brought to a full ignition by the battery, and kept ignited for a few seconds; connection was then broken and the lamp removed, so that the water gradually ascended. A bubble of the size of a large mustard-seed was left in the extremity of the tube, and I was much gratified at finding that when this was caught by a lighted match at the surface of the water trough it detonated. The experiment was then repeated, continuing the ignition for a longer time, but the gas could not be increased beyond a very limited quantity; indeed it was not to have been expected, as, supposing it to be mixed gas, re-combination of the excess would have taken place, and the fact of any uncombined gas existing when exposed to incandescent platinum will doubtless surprise those who hear it for the first time.

Fig. 7.



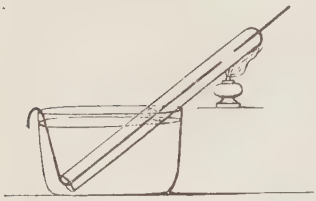
The experiment was repeated as at first and the bubble transferred to another tube; the wire was then again ignited in vapour, another bubble was instantly formed and transferred, and so on, until after about ten hours' work sufficient gas was collected for analysis; this gas was now placed in an eudiometer, it detonated and contracted to 0·35 of its original volume; the residue being nitrogen. The experiment was repeated several times with the same general results, the residue sometimes containing a trace of oxygen.

Here electrolysis was out of the question; the wire was ignited in (if I may use the expression) dry steam, the upper part of the tube being far above the boiling-point, and of course perfectly transparent; if not an effect of heat it must have been a new function of the electric current, at least one hitherto unknown,

As the voltaic arc and electric spark afford heat of the greatest intensity, I tried a succession of electric sparks from platinum wires through steam in the apparatus fig. 8, the water, as in all my experiments, having been previously purged of air (to save circumlocution I will in future call it prepared water). The sparks were taken from the hydro-electric

machine of the London Institution; they had in the steam a beautiful crimson appearance; on cooling the tube a bubble was perceptible, which detonated by the match.

Fig. 8.



As in the previous experiments, a whole day's work did not increase the bubble, but when it was transferred another instantly formed; the gas was similarly collected; it detonated and contracted to 0·4 of its original volume; the residue was nitrogen with a trace of oxygen.

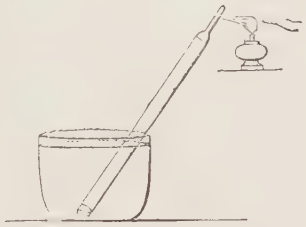
This experiment will again surprise by its novelty; the very means used in every laboratory to combine the mixed gases and form water here decompose water.* From a vast number of experiments which I have made on the voltaic and electric disruptive discharges (which are I believe similar phenomena, differing only in quantity and intensity), I believe the decompositions produced by them are the effects of heat alone, and this experiment was therefore to my mind a repetition of the last under different circumstances; others however may think differently. This experiment also I several times repeated.

By counting the globules given off, and comparing a certain number of them with the average volume of steam in the last two experiments, an attempt was made to ascertain what proportion of water could be decomposed by an ignited platinum wire in aqueous vapour, or, which amounts to a corollary from this proposition, what degree of dilution would enable mixed gas to exist without combustion in an atmosphere of steam exposed to an ignited platinum wire. The proportion in an experiment in which the globules were so counted was 1 in 2,400; the probability is however that different temperatures of the platinum wire would give different volumes of gas so decomposed, the volume being greater as the wire is more intensely ignited.

* I need scarcely point out the distinction, in fact, between this experiment and those in which liquid water has been decomposed by the electric spark. See Supplemental Paper.

Although there was no known effect of electricity which could produce the phenomenon exhibited by the last two experiments, and it was in any event new, still, firmly convinced that it was an effect of heat, I again determined to attempt its production by heat alone, and without the use of the battery. I procured a tube of silver 9 inches long and 0·4 inch diameter; at the extremity of this was a platinum cap, to which a smaller tube, also of platinum, was soldered. This platinum tube was closed at the end and soldered with gold solder. The apparatus was filled with prepared water; the water was boiled in the tube to expel the air from the narrow tube and any which might have adhered to the vessel; the tube was then, when full of hot water, inverted into water, and the flame of a common blowpipe made to play against the platinum tube (see fig. 9) until a white heat was obtained. Upon inverting it under water a bubble of the size of a mustard-seed rose to the surface, which gave a very feeble detonation with the match. Similar bubbles were collected as before, and the gas in an eudiometer contracted to 0·7. On repetition the experiment did not succeed so well, and upon several repetitions it sometimes succeeded and sometimes failed, and I should not mention it but that it was the first experiment which gave me, although not very satisfactorily, the effect of decomposition by heat alone. The reason of its uncertainty I believe to have been the want of a sufficiently intense heat, as I dared not venture, on account of the gold solder, to push the ignition very far; in fact, I subsequently fused the extremity and spoiled the apparatus by applying the oxyhydrogen flame to it; had the platinum tube been welded instead of gold-soldered, it would doubtless have succeeded better. I should state that the object of the silver tube was to prevent the chance of re-composition by the catalytic effect of a large platinum surface; to have, in short, a small portion of platinum exposed to the steam, and that at a high temperature.

Fig. 9.



This experiment, although, coupled with the previous ones, tolerably conclusive, did not satisfy me, and I attacked the difficulty in another manner. The experiment (fig. 5) induced me to believe that if I could get platinum ignited under water, so as to be in an atmosphere of steam, decomposition would take place; and M. Boutigny's experiments on the spheroidal state of water led me to hope I might keep platinum for some time under conditions suitable for my purpose.

After a few failures I succeeded perfectly by the following experiment. The extremity of a stout platinum wire was fused into a globule of the size of a peppercorn by a nitric acid battery of 30 cells; prepared water was kept simmering by a spirit-lamp, with a tube filled with water inverted in it; charcoal being the negative terminal, the voltaic arc was taken between that and the platinum globule until the latter was at the point of fusion; the circuit was now broken, and the highly incandescent platinum plunged into the prepared water: separate pearly bubbles of gas rose into the tube, presenting a somewhat similar effect to Experiment (fig. 5). The process was repeated, the globule being frequently plunged into the water in a state of partial fusion; and when a sufficient quantity of gas was collected it was examined; it detonated, leaving 0·4 residue; this was as usual nitrogen with a trace of oxygen. A second experiment gave a still better result, the gas contracting to 0·25 of its original volume.

On making the platinum negative and the charcoal positive a very different result followed; the carbon was, as is known to electricians, projected upon the platinum; and the gas in this case was mixed with carburetted hydrogen and carbonic oxide. I know no experiment which shows so strikingly the different effects at the disruptive terminals as this; when the platinum is negative it gives much carbonic gas, when it is positive, not a trace (the gas was delicately and carefully tested for it); nay, more, by changing the platinum from negative to positive the carbon is instantly removed, and in a single experiment the platinum becomes perfectly clean.

Here then I produced very satisfactorily decomposition by heat; it is true the battery was used, but used only as a means of

fusing the platinum, as this was, as soon as fused, entirely separated from the circuit and could have no possible voltaic action. Wishing, however, altogether to avoid the use of the battery, I repeated this experiment, employing as my means of fusing the platinum the oxyhydrogen blowpipe; the experiment was equally successful, indeed rather more so, as the manipulation was more easy.

I could readily by this means collect half a cubic inch or more of the gas; when detonated, the residue of nitrogen averaged 0·35 of the original volume.

In carefully watching this experiment I observed that at first a rapid succession of bubbles ascended into the tube from the incandescent platinum; it then became quiescent; the spheroidal state was assumed by the water and no gas ascended; on losing the spheroidal state a sudden hiss was heard, and a single bubble ascended into the tube. I determined to examine separately the gas from the platinum before and after the quiescent state; to effect this I placed two inverted tubes in the capsule, with the orifices near each other; the platinum at the point of fusion was immersed under one tube, say tube A, and as soon as the ascent of bubbles ceased it was removed across to tube B, and the last bubble then entered that tube; the gases from each tube were separately analysed, and tube A gave nearly all detonating gas, the residue being only 0·2; tube B gave none; the gas collected in it was nitrogen, with a trace of oxygen.

In order to examine the effect of an oxidable metal under similar circumstances, I fused by the oxyhydrogen blowpipe the end of a stout iron wire, plunged it into prepared water, and collected the globules of gas; no oxygen was given off, or at least no more than I have always found to accompany hydrogen, which, with a small residue of nitrogen, was the gas given off in this experiment.

I was now anxious to produce a continuous development of mixed gas from water subjected to heat alone, in other words, to succeed in an experiment which should bear the same relation to Experiment fig. 9 as fig. 5 did to fig. 7; for this purpose the apparatus shown at fig. 10 was constructed:

a and *b* are two silver tubes four inches long by 0·3 inch diameter; they are joined by two platinum caps to a platinum tube *c*, formed of a wire one-eighth of an inch diameter drilled through its entire length, with a drill of the size of a large pin; *a* is closed at the extremity, and to the extremity of *b* is fitted, by means of a coiled strip of bladder, the bent glass tube *d*. The whole is filled with prepared water; and having expelled the air from *a* by heat, the extremity of the glass tube is placed in a capsule of simmering water. Heat is now applied by a spirit-lamp, first to *b* and then to *a*, until the whole boils; as soon as ebullition takes place the flame of an oxyhydrogen blowpipe is made to play upon the middle part of the platinum tube *c*, and when this has reached a high point of ignition, which should be as nearly the fusing-point of platinum as is practicable, gas is given off, which, mixed

Fig. 10.

with steam, very soon fills the whole apparatus and bubbles up from the open extremity either into the open air or into a gas collector. Although by the time I had devised this apparatus I was from my previous experiments tolerably well assured of its success, yet I experienced a feeling of great gratification when, on applying a match to one of the bubbles which were ascending, it gave a sharp detonation; I collected and analysed some of it; it was 0·7 oxyhydrogen gas, the residue nitrogen, with a trace of oxygen.

Those who have endeavoured to deprive water of air will have no difficulty in accounting for the residual nitrogen, or nitrogen mixed with a small portion of oxygen, which has occurred in all my experiments. De Luc pointed out the impossibility of practically depriving water of air, and Priestley,

from observing the obstinacy with which water retained air, was led to believe that water was convertible into nitrogen (phlogisticated air). I have repeated several of Priestley's experiments under much more stringent circumstances, and have never been able to free water from air, or so to boil water that for every ebullition of vapour a minute bubble of permanent gas was not left, which appeared to have been an indispensable nucleus to the vapour.

The difficulty of boiling water increases, as M. Donny has proved, in proportion to its freedom from air, and at last the bursts of vapour become so enormous that the vessels employed are generally broken. There appears to me a point beyond which this resistance does not extend; but even at this point a minute bubble of air is left for each burst of vapour, though they are so few and distant that the aggregate amount of gas is very trifling. I have produced from water which had been previously carefully deprived of air by the ordinary methods three-fourths of its own volume of permanent gas, which proved to be nitrogen; but as the water in this experiment was boiled under a long column of oil, it is probable that if any oxygen were present it might have been absorbed by the oil; I have, however, always found the proportion of oxygen to decrease as the boiling was continued. It may be worth noticing, as having had some influence on my mind, that many months ago, when considering the experiments of Henry and Donny on the cohesion of water, I mentioned to Mr. Gassiot, and also to Mr. Bingham, my assistant (to whose assiduity I am much indebted), that I was inclined to think if water could be absolutely deprived of air it would be decomposed by heat, a result which I have now attained by a totally different series of inductions. It is a circumstance worthy of remark, that I find the greater part of the air to be expelled at a comparatively low temperature, and when the water has come in contact with the platinum, while the decomposition all takes place when the platinum is surrounded by an atmosphere of steam, if steam it may be called, for the state of this atmosphere at the first immersion of the platinum is at present very mysterious.

I think I may now safely regard it as proved that plati-

num intensely ignited will decompose water, and several considerations press on the mind in reflecting on this novel phenomenon.

First of all, to those who are attached to the *cui bono* argument, and estimate physical science in proportion only to its practical applications, I would say that these experiments afford some promise of our being, at no distant period, able to produce mixed gases for purposes of illumination, &c., by simply boiling water and passing it through highly ignited platinum tubes, or by other methods which may be devised; we in fact by this means, as it were, boil water into gas, and there appears theoretically no more simple way of producing chemical decomposition.

To pass however to more important considerations: the spheroidal state, which has lately attracted the attention of philosophers, appears to be closely connected with these results, and is rendered more deeply interesting. The last experiment but two which I have mentioned shows that the spheroidal state is intermediate between ordinary ebullition and the decomposing ebullition. It is probably therefore a state of polar tension, co-ordinate in some respects with that which takes place in the cell of a voltaic combination before decomposition, or when the power employed not being of sufficient intensity to produce actual decomposition, the state commonly called polarisation of the electrodes, obtains. The phenomenon brings out also a new relation between heat, electricity, and chemical affinity; hitherto many electrical phenomena could be produced by heat and chemical action, the difference being that in the effects produced by the last two forces there was no polar chain, but every minute portion of the matter acted on gave rise to the phenomena which in the electrical effects are only observable at the polar extremities; thus in decomposing water by iron and sulphuric acid, or by passing steam over heated tubes of iron, parallel results are obtained to the electrolysis of water with an iron anode; but in the former cases every portion of the iron oxidated gives off its equivalent of hydrogen; in the latter the equivalent is evolved from the cathode at a point distant from that where the oxidation takes place. Hitherto electricity has been the only

force by which many compounds, and particularly water, could be resolved into their constituents without either of these being absorbed by another affinity. The decomposition by ignited platinum removes this exception, and presents the parallel effect produced by heat alone.

Although there is no substance except platinum and some of the more rare metals, such as iridium, which promise much success in a laboratory experiment made for the purpose of producing the effect I have described, as the greater number of substances which will bear a sufficient heat are fragile, oxidable, or affected by water, yet general considerations from the nearest analogies in chemistry would lead us to expect a similar effect from all matter in a state of intense ignition; even assuming the presence of solid matter to be necessary, the catalytic effects of platinum are shared in different degrees by other substances; it therefore appears probable that at a certain degree of heat water does not exist as water or steam, but is resolved into its constituent elements. If, therefore, there be planets whose physical condition is consistent with an intense heat, the probability is, that their atmosphere and the substances which compose them are in a totally different chemical state from ours, and resolved into what we call elements, but which by intense heat may be again resolved into more subtle elements. The same may be the case in the interior of our planet, subject, however, to the counter-agency of pressure.

The experiments strongly tend to support the views of Berthollet, that chemical and physical attraction are affinal, or produced by the same mode of force. All calorific expansions appear to consist in a mechanical repulsion of the molecules of matter; and if heat produce effects of decomposition merely by increase of intensity, there seems no reason why we should assign to it in this case a different mode of action from its normal one. On this view physical division carried on indefinitely must ultimately produce decomposition, and chemical affinity is only another mode of molecular attraction. Thus a high degree of rarefaction, as at the bounds of the atmosphere, or in the interplanetary spaces, may entirely change the chemical condition of matter.

In a paper published in the 'Philosophical Transactions' for 1843, p. 111, I have shown that we may oppose a chemical action by a physical one (electrolysis by a vacuum), that antagonising chemical by physical tension, they mutually oppose each other. I believe the converse of this experiment has been made by M. Babinet, who by physical compression has prevented the development of chemical action.

I have also described in the 'Philosophical Magazine' for November 1845 certain phenomena which appear to me to be irreconcileable with received chemical views; and though I then believed that the theory of Grotthus would be obliged to give way, I now incline to think that some of our chemical doctrines must ere long undergo a revision.

It is rather surprising that the valuable applications of which the phenomena of voltaic ignition are capable, and the fertile field which (as I believe) it presents for discoveries, both physical and chemical, should have been so completely neglected. It is true that, until a recent period, the imperfection of the voltaic battery rendered accurate and continued experiment on this subject difficult of performance, but still much might have been done. Davy made several experiments on the voltaic disruptive discharge, which, in many points, may be regarded simply as very intense ignition; but I am only aware of two experiments of his on voltaic ignition; one, in which he employed it in an exhausted receiver to examine to what extent the radiation of heat was carried on *in vacuo*; and another, already alluded to, in which, by immersing a portion of an ignited wire in water, he observed that it conducted in some inverse ratio to its heat.

I have made a vast number of experiments on the voltaic arc or disruptive discharge, in various media;* when this is taken in a medium incapable of acting chemically on the electrodes, the phenomena are those of intense ignition of the terminals, which are dissipated in vapour and condensed upon the interior of the vessel in which the discharge is taken. I have examined some of these deposits, and they appear to consist of the metal of the terminals in a finely-divided state;

* *Phil. Mag.*, June 1840; *Lit. Gaz. and Athenæum*, Feb. 7, 1845.

this is strikingly shown with zinc. If the arc be taken between zinc points in an exhausted receiver, a fine dark powder, nearly black, is deposited on the interior, which, when collected, proves to be pure zinc, and on the application of a gentle heat takes fire in the open air and burns into the white oxide: to casual observation the zinc would appear to be burned twice. The experiment appears to me to present an argument in favour of the dynamic theory of heat.

With charcoal, on the other hand, there is little or no deposit, but the charcoal continually yields carbonic oxide and hydrogen, and this for hours after the presence of water would be deemed impossible. I have taken the arc between pieces of well-burned charcoal for eight or nine successive hours, and there was still gas generated; indeed, it appeared to be given off as long as there was any charcoal remaining, and a conversion of the carbon into inflammable gas might have been supposed. Much still remains to be done with this powerful agent, the voltaic arc: where, however, the object is simply to expose gases to an intense heat, the ignition of a conjunctive wire of platinum is more simple in its application, more uniform in its action, and instead of requiring a powerful battery, the effect can be satisfactorily produced by five or six cells, in many cases by two.

The heat is not so intense as that of the arc, but as it can be brought to within a few degrees of the fusing-point of platinum, it is far more intense than any heat usually employed in laboratories, certainly than any which can be applied to minute, I may say microscopic portions of gas or vapour.

In conclusion, I must express my sincere thanks to the Managers of the London Institution, for having permitted me, as an honorary member, to carry on these experiments in the laboratory of the Institution.

SUPPLEMENTARY PAPER ON CERTAIN PHENOMENA OF VOLTAIC IGNITION, AND THE DECOMPOSITION OF WATER INTO ITS CONSTITUENT GASES BY HEAT.

Phil. Trans., R.S. *Received* November 26. *Read* November 26, 1846.

In selecting the above title I endeavoured to give as clear an enunciation of the phenomena to be described in the paper as was consistent with the brevity usual in a title.

An exception has, however, been taken to it, that as the effects of decomposition are produced by ignited platinum, the phenomena may result from that obscure mode of action called catalysis. That I did not intend to exclude from consideration any possible action of the substance employed will be evident from the paper itself, in which I have called attention to the general production of catalytic effects by solid bodies.

Whatever value or novelty there may be in the facts I have communicated is the same, whether they be regarded as resulting from catalytic or from thermic actions. If the action be catalytic, it is one absolutely the reverse of that usually produced by platinum, and therefore just as much at variance with received experience as decomposition of water by heat would be, the effect of platinum, like that of heat, on the elements of water having been hitherto known only as combining them. With regard to any theoretic views I may have advanced, I by no means attach the same importance to them as I do to the facts themselves, though I consider it necessary for the collation of facts, and desirable for the progress of science, that an author pretending to communicate new results should give with them the impressions which led to their discovery, and the inferences which he regards as immediately deducible from them. No expression can be given to facts which does not involve some theory; and admitting the difficulty (perhaps insuperable) of correctly enunciating new phenomena, and the probability of future discoveries entirely

changing our views regarding them, I cannot at present see that the title of my paper could be altered without being open to greater objections. I am of this opinion, not so much because other bodies than platinum will produce the effect, as I shall presently show, nor from the fact that the electrical spark will decompose aqueous vapour, though these are arguments in its favour, but from the following considerations. The catalytic action of platinum will induce or enable combination to take place where there is already a strong affinity or tendency to combine, as with mixed oxygen and hydrogen gases; it will also induce decomposition where the affinities are extremely weak, or in a state of unstable equilibrium, as in Thenard's peroxide of hydrogen; again, where there are nicely-balanced compound affinities, it may change the chemical arrangement of the constituents of a compound, but I do not know of any case in which a powerful chemical affinity can be overcome by catalytic action; to effect this we require some natural force of greater intensity than that to be overcome. We might as well say that the platinum electrodes of a voltaic battery decompose water, as to say that platinum decomposes it in the case in question; there the force of electricity acts only by means of matter, and matter of a peculiar description; its action also is only perceptible at the surface of this matter. I seek to use the expression in my title with reference to heat in a similar sense to that in which we use similar terms with reference to electricity, *i.e.* to regard heat as the immediate dynamic force which overcomes the affinity; thus, as we say when employing the voltaic battery, that we decompose water by electricity, so here we should say that we decompose it by heat.

If it be said that heat so weakens or antagonises the affinity of the elements of water as to enable catalytic action to separate them, this amounts to the same theory, as heat is then regarded as the antagonising force, and in this case the action, both thermic and catalytic, is the reverse of the normal action. I have thought it desirable shortly to discuss this question, as likely to lead to farther investigation, though I have been somewhat embarrassed by the want of definite meaning in the term catalysis; I must plead guilty to have

frequently used the term ; but notwithstanding, or perhaps on account of, its convenience, it has I fear had an injurious effect on scientific perspicuity.

The following experiments were made to ascertain whether platinum was the only substance by which the effect could be produced. A knob or button of the native alloy of iridium and osmium of the size of a small pea was formed by the voltaic battery ; to this was attached by fusion another smaller knob of the same metal one-fourth the size of the former, and to this smaller one was attached a stout platinum wire ; the object of the second knob was both to prevent the fusion of the platinum wire, and also to avoid the possibility of any of the platinum being exposed under the recipient tube or alloyed with the metal to be heated. The preparation of this simple instrument was very troublesome, but when made it answered the purpose well ; the larger button could be fully ignited to an intense glow, while, on account of the narrow neck which united them, the smaller was barely red-hot, and the platinum wire not perceptibly ignited. An experiment having been made with this metallic button and prepared water, similar to that previously made with platinum, gas was given off which averaged 0·3 of mixed gas ; the residue was nitrogen mixed with varying small quantities of oxygen. The effect, upon the whole, was decidedly inferior to that of the platinum. Indeed, as platinum is the most dense and unalterable of all known substances, it would be likely, upon any received theory of heat, to produce the greatest effects.

I tried palladium in the same manner ; the gas yielded was hydrogen with small quantities of oxygen, and the water was stained with the oxide of the metal.

I now tried silica and other oxides, but the results were not very satisfactory. A spheroid of silica was formed by fusing pulverised silica on to a platinum wire, so as to cover it for the length of 0·4 of an inch ; when this was plunged into the hot water and again fused in the oxyhydrogen blow-pipe, it constantly became frothed with small bubbles of vapour, and after a few experiments generally separated in fissures ; in the experiment which was continued for the

longest time without disintegration, the gas given off contained 0·15 of oxyhydrogen gas; from the whole result I believe there is an action of the water on the silica (probably forming a hydrate decomposable by heat) which is a bar to satisfactory results. With other oxides, at least such as would bear an intense heat, the difficulties were still more insuperable. Priestley has shown that water will corrode glass, and if I mistake not, others have shown the same effect produced on silica.

Although, as applied to the facts detailed, I attached no further meaning to the title of my paper than that which I have above stated, yet in one or two theoretical inferences I have certainly gone farther; for instance, when I suppose the possibility or probability of mechanical rarefaction producing the same effects as heat, here (although I do not, indeed I cannot conceive the existence of heat without matter) I certainly abstract from the proposition any consideration of solid matter. In order to ascertain how far this view might be founded on truth, I had thought of making a few experiments on the effect of mechanical rarefaction on the tendency of gases to combine, but (in addition to the interference of necessary occupations) I find that M. de Grotthus has already experimented on the point. His experiments, as far as they go, corroborate the views I have put forth.

He finds * that mixed gases, such as chlorine and hydrogen, or oxygen and hydrogen, when rarefied either by slow increments of heat or by the air-pump, do not take fire ('ne s'enflamment pas') by the electric spark. From the context he evidently means that the gases will not detonate or unite in volumes, as he states that a partial combination ensues. Grotthus appears to have considered the combination of gases by the electric spark as an effect of sudden compression or molecular approximation, certain particles being brought within the range of their affinities by the sudden dilatation of others. Although he did not pursue the subject far enough to ascertain whether a degree of rarefaction could be reached which would be an actual bar to combination, still his experiments strengthen those views which assimilate mechanical and

* *Annales de Chimie*, vol. lxxxii.

thermic molecular repulsion, and regard chemical affinity as being antagonised by physical repulsion.

Pursuing the series of analogies from the decomposition of euchlorine at a low temperature, that of ammonia at a higher, that of metallic oxides at a higher, and so on to oxide of hydrogen, there appears to be an extensive series of facts which afford strong hope of a generalised antagonism between thermic repulsion and chemical affinity, and a consequent establishment of the law of continuity in reference to physical and chemical attraction.

The deposit from chlorine, to which I have alluded in my paper, I have since examined, and though it differs in colour from that described in books, I find it is a protochloride of platinum, formed at the expense of the platinum wire. The larger portion of the chlorine in the tube combines with the hydrogen of the aqueous vapour, and the muriatic acid is absorbed by the water; when the experiment terminates the gaseous volume is reduced to nearly one-half, and this residue is oxygen.

This effect induced me to try an ignited wire on other analogues of chlorine, and I tried bromine and chloride of iodine in the apparatus (fig. 5). The tube was filled with the liquid, and its extremity was in the first experiments immersed in another narrow tube of the same liquid as that which filled it. When the platinum wire was ignited permanent gas was given off, both from the bromine and from the chloride of iodine, which gas on examination proved, to my surprise, to be oxygen. In one experiment I collected half a cubic inch of gas from an equal volume of chloride of iodine. As the experiment in this form required too large a quantity of the liquid to enable me to observe any change which might take place in its character, I repeated it with a tube five feet long, bent in two angular curves. A small quantity of the liquid was placed in the extremity of the tube containing the wire, which was so arranged as to be the lowest point; the angles were placed in cold water and the experiment proceeded with; my object was to enable the dense vapour of the liquids to shelter them from the atmosphere, there being no satisfactory method of shutting them in and yet allowing

room for the elimination of the liberated gas, or of absorbing the latter by combination without also absorbing the vapours.

I had hoped by the above means to proceed with the experiments until all the oxygen was liberated that could be driven off, and then to have examined the residue; but I found that, after experimenting for a short time, both the platinum wire and the glass in proximity to it were attacked by the liquids; this difficulty, similar to those which have hitherto prevented the isolation of fluorine, I have not yet been able to conquer, though I hope to resume the experiments.

As chloride of iodine is decomposed by water, it cannot contain any notable quantity of the latter, but, until the experiments are carried farther, it must remain a question whether the oxygen results from a small quantity of water contained in the liquid, the hydrogen combining with the liquid itself, or from a decomposition similar to that of the peroxides. The experiments certainly add a new and striking analogy to those already known to exist between the peroxides and the halogens, but they do not, as far as I have hitherto carried them, necessarily prove analogy of composition.

In conclusion, I would call attention to a point which I omitted to notice in my original paper, viz. the explanation afforded by the results contained in it of the hitherto mysterious phenomena of the non-polar decomposition of water by electrical discharges, as in the experiments of Pearson and Wollaston. This class of decompositions may now be carried much farther. With the exception of fused metals, I know of no liquid which, when exposed to intense heat, such as that given by the electric spark, the voltaic arc, or incandescent platinum, does not give off permanent gas; phosphorus, sulphur, acids, hydrocarbons, water, salts, bromine, and chloride of iodine, all yield gaseous matter.

Viewing these effects simply as facts, and without entering on any theoretical explanations or speculations, I cannot but think that there is a remarkable generality pertaining to them worthy of the most careful attention.

The apparatus I have described, particularly that represented by fig. 5, and the numerous applications of voltaic

ignition which will occur to those who duly consider the subject, promise, I venture to believe, new methods and powers of investigating the molecular constitution of matter, and will, I trust, lead to novel and important results.

ON THE EFFECT OF SURROUNDING MEDIA ON VOLTAIC IGNITION.

Phil. Trans. R. S. Received August 10. Read December 14, 1848.

In the 'Philosophical Magazine' for December 1845 I pointed out a striking difference between the heat generated in a platinum wire by a voltaic current, according as the wire is immersed in atmospheric air or in hydrogen gas, and in the Bakerian Lecture for 1847 I have given some farther experiments on this subject, in which the wire was ignited in atmospheres of various gases, while a voltameter enclosed in the circuit yielded an amount of gas in some inverse ratio to the heat developed in the wire. It was also shown, by a thermometer placed at a given distance, that the radiated heat was in a direct ratio with the visible heat.

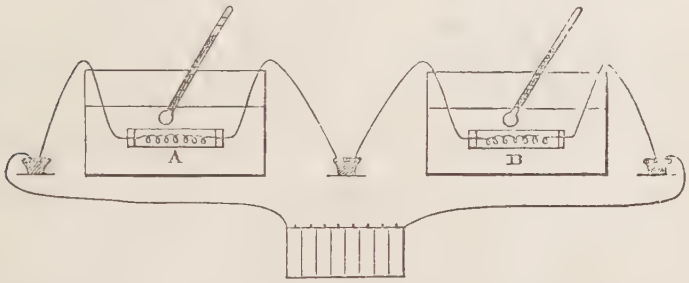
Although the phenomena were apparently abnormal, there were many known physical agencies by which they might possibly be explained, such as the different specific heats of the surrounding media, their different conducting powers for electricity, or the varying fluency or mobility of their particles which would carry off the heat by molecular currents with different degrees of rapidity.

The investigation of these questions will form the subject of this paper.

An apparatus was arranged (see fig. 1). Two glass tubes, A and B, of 0·3 inch internal diameter and 1·5 inch length, were closed with corks at each extremity; through the corks the ends of copper wires penetrated, and joining these were coils of fine platinum wire, one-eightieth of an inch diameter and 3·7 inches long when uncoiled. Tube A was filled with oxygen, tube B with hydrogen, and the tubes thus prepared

were immersed in two separate vessels, in all respects similar to each other, and containing each three ounces of water. A thermometer was placed in the water in each vessel; the copper wires were connected, so as to form a continued circuit, with a nitric acid battery of eight cells, each plate exposing eight square inches of surface. Upon the circuit being completed the wire in the tube containing oxygen rose to a white heat, while that in the hydrogen was not

Fig. 1.



visibly ignited; the temperature of the water, which at the commencement of the experiment was 60° Fahr. in each vessel, rose in five minutes in the water surrounding the tube of hydrogen from 60° to 70°, and in that containing oxygen from 60° to 81°.*

Before I enter into a farther detail of experiments I would remark upon the extraordinary character of this result. The same current or quantity of electricity passes through two similar portions of wire immersed in the same quantity of

* After the publication of the Bakerian Lecture my experiment on the peculiar effect of hydrogen on the ignited wire was noticed in a paper by M. Matteucci, which, though I had it in my hand shortly after its publication, I regret to say I did not read with the attention it deserved. I have read it since the experiments in this paper were commenced, and I see that I am now executing a task assigned to me by my friend. M. Matteucci, for a different object, makes a somewhat similar experiment to the one given above, which, however, differs from mine in the material point, that he operated first on one gas and then on the other, and thus did not compare the effects produced by the same quantity of electricity. I cannot quite agree in the conclusions deduced by him from this and the other experiments he cites, but I will not here contest them, as it would lead me away rom the main point of this paper.

liquid, and yet, in consequence of their being surrounded by a thin envelope of different gases, a large portion of the heat which is developed in the one portion appears to have been annihilated in the other. Similar experiments, varying the gas in one tube while hydrogen was retained in the other, gave the following results. In five minutes the thermometer rose—

	In hydrogen.	In associated nitrogen.
1st.	From 60° to 69·5°.	From 60° to 81·5°.
	In hydrogen.	In carbonic acid.
2nd.	From 60° to 70·5°.	From 60° to 80°.
	In hydrogen.	In carbonic oxide.
3rd.	From 60° to 70°.	From 60° to 79·5°.
	In hydrogen.	In olefiant gas.
4th.	From 60° to 70·5°.	From 60° to 76·5°.*

On a different day I tried the following experiments; all the circumstances were the same, excepting that the battery was in more energetic action, for which reason I have not tabulated them with the others.

In oxygen associated with coal gas the thermometer rose in five minutes—

In oxygen.	In coal gas.
From 60° to 82°.	From 60° to 76°.

In hydrogen associated with coal gas the thermometer rose in five minutes—

In hydrogen.	In coal gas.
From 60° to 77°.	From 60° to 82·5°.

From this it would appear that coal gas should be placed, as to its cooling effect on the ignited wire, between hydrogen and olefiant gas.

On another day sulphuretted hydrogen associated respectively with oxygen and hydrogen was tried; the wire in the

* I should perhaps remark, that several test experiments were tried to ascertain the working of the apparatus; thus, the same gas was placed in both tubes, and the results given by the thermometer were found to be accurately the same in both vessels. The tubes were also changed with reference to the containing vessels and to the contained gases. The water was always agitated to render its temperature uniform previously to reading off, &c.

sulphuretted hydrogen was at first ignited to a degree somewhat inferior to that in oxygen, but the gas was rapidly decomposed; sulphur being deposited on the interior of the vessel and the intensity of ignition gradually decreased, so as ultimately to be scarcely superior to the ignition in hydrogen; indeed the gas by this time had become nearly pure hydrogen. The following were the effects on the thermometer in five minutes, all being arranged as before :—

In oxygen.	In sulphuretted hydrogen.
From 60° to 86°.	From 60° to 76°.
In hydrogen.	In sulphuretted hydrogen.
From 60° to 79°.	From 60° to 81·5°.

This result would place sulphuretted hydrogen between hydrogen and coal gas; but as the gas was rapidly decomposed, the greater part of the experiment was made with hydrogen containing small quantities of sulphur combined, and not with sulphuretted hydrogen. I therefore think that proto-sulphuret of hydrogen, or the gas which consists of equivalent ratios of the two elements, would be much farther removed from pure hydrogen; probably it would be about equal in its cooling effect to carbonic acid or carbonic oxide.

In phosphuretted hydrogen the platinum wire is destroyed by combining with the phosphorus the instant it reaches ignition, so that its relation to the other gases could not be ascertained.

Protoxide and deutoxide of nitrogen are, as I have observed in the Bakerian Lecture, decomposed by the ignited wire; they, as well as atmospheric air, are, as nearly as may be, equal in their effect to their elements separately.

In the vapour of ether the ignited wire is extinguished nearly as completely as in hydrogen; I have not yet tried its comparative effect, but should judge it to be nearly the same as coal gas or olefiant gas.

In my former experiments* the following was the order of the gases, testing the intensity of ignition by the inverse con-

* *Phil. Trans.*, 1847, p. 2.

ducting power of the wire, as measured by the amount of gas in a voltameter included in the circuit :—

Gases surrounding the wire.	Cubic inches of gas evolved in the voltameter per minute.
Hydrogen	7·7
Olefiant gas	7·0
Carbonic oxide	6·6
Carbonic acid	6·6
Oxygen	6·5
Nitrogen	6·4

Assuming that in the present experiments the heat in the water is a correct indication of the intensity of ignition in the wire, the order is the same in both series of experiments. Hydrogen is, however, so far removed from both oxygen and nitrogen in its effects upon the ignited wire, that in order more accurately to ascertain the relative position of the latter two gases, I made a few farther experiments on them as contrasted with each other, and not with hydrogen. I first repeated my former experiment on these two gases, varying it only by changing the circumstances in the manner suggested by the present experiments, which, on account of the vessel containing the wire being immersed in a given quantity of water, instead of being exposed to the external atmosphere, would occasion greater equality in the surrounding cooling effects, and would give me the opportunity of combining both methods in one experiment.

I filled both tubes A and B with oxygen, and included a voltameter in the circuit ; in two minutes 3·43 cubic inches of hydrogen were evolved in the voltameter, and the thermometer in each cell had risen from 60° to 63°. A similar experiment with nitrogen gave in two minutes 3·4 cubic inches of hydrogen, and the thermometer rose from 60° to 63°.

This experiment accords with my previous one as to the voltameter test, but indicates no difference in oxygen and nitrogen with the thermometer test ; I therefore in the following three experiments associated nitrogen with oxygen in the apparatus (fig. 1). All things being disposed as with the experiments on hydrogen associated with other gases, in five minutes the thermometer rose—

	In oxygen.	In associated nitrogen.
Exp. 1st.	From 60° to 71·5°.	From 60° to 73°.
„ 2nd.	„ 60° to 77°.	„ 60° to 76°.
„ 3rd.	„ 60° to 75°.	„ 60° to 76°.
Mean . .	60° to 74·5°.	„ 60° to 75°.

The battery had increased somewhat in power after the first experiment; but as both wires formed part of the same circuit in each experiment, the variations in battery power do not affect the comparative results. The second experiment gives a variation in the position of oxygen and nitrogen with reference to the first and third experiments, but the gases so nearly approach in their cooling effects, that these slight differences are not much to be relied upon; however, I applied a farther test. I associated in turn oxygen and nitrogen with carbonic acid; the following were the results. In five minutes the thermometer rose—

	In oxygen.	In carbonic acid.
Exp. 1st.	From 60° to 75°.	From 60° to 75°.
2nd.	„ 60° to 76°.	„ 60° to 75°.
	In nitrogen.	**In carbonic acid.**
Exp. 1st.	From 60° to 74°.	From 60° to 73°.
2nd.	„ 60° to 73°.	„ 60° to 72·5°.

The battery had in the last experiment a little decreased in power; the oxygen and nitrogen both produced a less cooling effect than the carbonic acid, but the oxygen came nearer to it than the nitrogen, thus according with the previous experiments. Upon the whole it would appear that oxygen produces a somewhat greater cooling effect on the ignited wire than nitrogen, but these gases may, for the purposes of this paper, be fairly regarded as equal. Atmospheric air produces a similar effect to oxygen and nitrogen separately, though I am inclined to think that a slight chemical change takes place when atmospheric air is exposed to the ignited wire, and that nitrous acid is formed; for if litmus-paper be held over a voltaically ignited platinum wire in the air, a slight but very perceptible tinge of red marks the portion of it immediately over the wire.

With the view of ascertaining whether the specific heat of

the surrounding media were the cause of the phenomenon, I proceeded to try the effect of the wire carrying a voltaic current on different liquids; all things being disposed as in the previous experiments, and three ounces of water being associated respectively with the same quantity of the following liquids. The thermometer rose in five minutes—

In water, from 60° to 70°·3.	In spirit of turpentine 60° to 88°.
In water, from 60 to 70·3.	In sulphuret of carbon 60 to 87·1.
In water, from 60 to 69.	In olive oil . . . . 60 to 85.
In water, from 60 to 70·1.	In naphtha . . . . 60 to 78·8.
In water, from 60 to 70·5.	In alcohol sp. gr. 0·84, 60 to 77.
In water, from 60 to 68·5.	In ether . . . . . 60 to 76·1.

I do not much rely on the last experiment—the battery was in more feeble action; and though each of the above results is the mean of three experiments, yet the variations in the results of the different experiments with ether being considerable (while in the others they were very trifling), lead me to place no great dependence on it. The rapidity of evaporation and the readiness of ebullition of the ether require that a larger quantity should be used; but as this for the purpose of comparison would have required all the experiments to be repeated with different quantities of liquid, I have not thought it worth while to go through the series a second time. It will be observed that the effects with the above liquids are by no means in direct relation with their respective specific heats; but in order to bring the results of the experiments with liquids into comparison with those with gases, I now associated a gas with a liquid, viz. hydrogen with water. All things being disposed as before, the tube A was filled with hydrogen gas, the tube B with water, both being immersed in three ounces of water. The thermometer rose in five minutes—

In hydrogen.	In water.
From 60° to 75·5.°	From 60° to 72°.

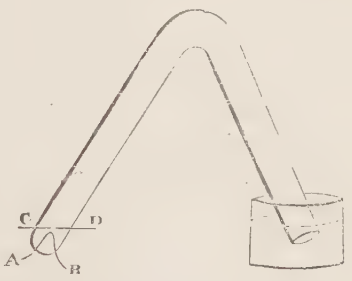
This experiment of itself conclusively negatives the possibility of specific heat alone accounting for the phenomenon under consideration; and though, doubtless, specific heat must have some influence on the cooling effects of different

gases and liquids, yet in the former it is apparently of very trifling import in comparison with the real physical cause of the differences, whatever that may be.

Supposing, as is stated by Faraday,* that gases possess feeble conducting powers for voltaic electricity, and supposing hydrogen, from its close analogy in chemical character to the metals, to possess a greater conducting power than the other gases, this would account for its peculiar effect on the ignited wire, as a certain portion of the current, instead of forcing its way through the wire, would be carried off by the surrounding gas. In order to ascertain this I arranged the following experiments.

1st. Into the closed end of a bent tube, fig. 2, a loop of platinum wire, A B, and two separate platinum wires, C D, were hermetically sealed, the extremities of the latter being approximated as closely as possible, and the interval between them being close to and immediately over the apex of the loop. The tube was filled with hydrogen, and the wire A B connected with a voltaic battery of sufficient power to raise it to as high a degree of ignition as it would bear without fusion; C and D were now connected with the poles of another battery, a delicate galvanometer being interposed in the circuit. Not the slightest effect on the galvanometer needle could be detected, and a similar negative effect took place when the tube was filled with atmospheric air.

Fig. 2.



2nd. Parallel portions of platinum wire were now arranged in close proximity (see fig. 3), and so that each might be ignited to a full incandescence by separate insulated batteries. When surrounded by atmospheres, both of atmospheric air and of hydrogen, and fully ignited, not the slightest conduction could be detected across the interval between the wires, with ten cells of the nitric acid battery; and being enabled by

* *Experimental Researches*, §§ 272, 441, and 444.

the kindness of Mr. Gassiot to repeat this experiment with his battery of 500 well-insulated cells of the nitric acid combination, air did not conduct when the ignited wires were approximated to the $\frac{1}{50}$th of an inch; on approaching them nearer they came within striking distance, were instantly fused, and the galvanometer needle, which had up to this time been perfectly stationary, was whirled rapidly round.

Fig. 3.

I think I am entitled to conclude from this that we have no experimental evidence that matter in the gaseous state conducts voltaic electricity; probably gases do not conduct Franklinic electricity, as the experiments which would seem *primâ facie* to lead to that conclusion are explicable as resulting from the disruptive discharge.

In Faraday's experiment two wires were approximated in the flame of a spirit-lamp, and a slight conduction across the interval in the flame was observed. This conduction might have been due to certain unconsumed particles of carbon existing in the flame, or possibly to the flame itself. According to Dr. Andrews, flame, even that of pure hydrogen gas, conducts voltaic electricity.*

I now endeavoured to ascertain whether any specific inductive effect of the hydrogen might have an influence: parallel wires of platinum and parallel coiled copper wires were placed in atmospheres of hydrogen and of atmospheric air, one of which parallel wires conveyed the current, and the other wire was connected with a delicate galvanometer.† I could detect no difference in the arcs of deflection of the needle at the instant of meeting or breaking contact, whether the wires were in atmospheres of hydrogen or of atmospheric air; nor when parallel platinum wires, with their surrounding atmospheres of gas, were immersed in a given quantity of water,

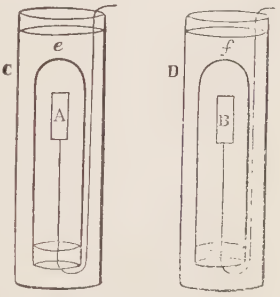
* *Phil. Mag.*, vol. ix., p. 176.

† I have obtained a slight effect on the galvanometer in a circuit with interposed parallel wires ignited in the vacuum of a good air-pump. *Quære*, whether this is true conduction or a disruptive effect.—W.R.G., 1874.

could I detect any difference in the resulting heat, whether the current passed in the same or in a different direction through each wire respectively.

My next object was to ascertain whether, in cases of ordinary ignition, the same apparent annihilation of heat took place in hydrogen gas as with voltaic ignition. Two iron cylinders, A B, fig. 4, each weighing 390 grains, were attached to long iron wires bent back in the form shown in the figure. The cylinders were placed together in a crucible of fine sand, and the whole heated to a uniform white heat. The cylinders were now taken out of the sand, placed at the surface of equal portions of water in the vessels C and D; two inverted tubes, *e*, *f*, the one of hydrogen, the other of atmospheric air, were placed over them, and the whole quickly immersed in the water, and retained by a little contrivance, which I need not particularise, in the position shown in the figure. The temperature of the water at the commencement of the experiment was 60° Fahr. In four minutes the water surrounding the hydrogen had risen to 94°, and became stationary there, while that surrounding the air had only reached 87°; in ten minutes the water surrounding the hydrogen had sunk to 92·5°, while that surrounding the air had risen to 93°, which was the highest temperature it reached; thus the respective maxima were 94° and 93°; but considering the greater time which the water surrounding the air required to attain its maximum tempera- [illegible] nd that, being during this time at a temperature above that of the surrounding atmosphere, it must have lost something of its acquired heat, we may fairly consider the maxima to be the same, and that the difference of effect in the two gases had reference solely to the time occupied in the transference of the heat. In a second experiment the results were similar, the maximum being in this experiment 92·5° in hydrogen, and 91° in air.*

Fig. 4.



* Iron wire produces a similar effect to platinum wire in the voltaic experiments.

As far as ordinary ignition is concerned, hydrogen has been shown by the experiments of Leslie and Davy to produce a more rapid cooling effect than air ; and the above experiment having shown that it does not alter or convert into any other force the actual amount of heat given off, my next step was to enquire whether this rapidity of cooling effect of the hydrogen would account for the effects observed with voltaic ignition. Although the two classes of effects were apparently very different, it might be that the improved power of conduction in the wire arising from the rapid cooling effect of the hydrogen might, by enabling the current to pass more readily, carry off the force in the form of electricity, which if the wire offered more resistance (as it would when more highly ignited) would be developed in the form of heat. By employing the same medium, but impeding the circulation of the heated currents in one case, while their circulation was free in the other, some light might be expected to be thrown on the inverse relation of the conducting power to the heat developed. The following experiment was therefore tried :—

In the apparatus represented in fig. 1, tube A was uncorked, so as to allow free passage for the water, while tube B was filled up with fine sand soaked with water, and then corked at both ends ; the current was passed, and the following was the result. In the vessel containing tube A the thermometer rose in five minutes from 52° to 60°, and in that containing tube B from 52° to 60° also ; during a second five minutes the thermometer rose in the vessel containing A from 60° to 67°, and in the vessel containing B from 60° to 67° also.

I tried another analogous experiment : a coil of platinum wire was placed in a very narrow glass tube one-sixth of an inch diameter ; this was hermetically sealed at one end, and the other drawn into a very narrow aperture, little more than sufficient to allow the platinum wire to pass, and filled with water (it was necessary to leave a small aperture, to prevent the bursting of the tube by the expansion of the heated water) ; in the other vessel a similar coil of platinum wire was placed, but without any glass tube at all. The circuit having been completed as before, the thermometer rose in five minutes—

In the water without the tube, from . . 60° to 87°.
In the water containing the tube, from . 60° to 86°.

Here the difference, slight as it was, was against what theory would have led one to anticipate; the exact equality, however, of the previous experiment, and the close approximation of the results in this one, afford no conclusive information as to the point under consideration, though the negative result rather tends against the view which would assimilate the effects of voltaic to those of ordinary ignition.

As another method of attaining the object before mentioned, viz. the inverse relation of the conducting power of the wire to the heat developed in it, I tried the following experiment. A platinum wire of one foot long and $\frac{1}{80}$th of an inch diameter was ignited in air by ten cells of the battery, a voltameter being included in the circuit; the amount of hydrogen given off by the voltameter was one cubic inch in forty-four seconds: half the wire was now immersed in water of the temperature of 60° Fahrenheit; by this means the intensity of ignition of the other half was notably increased; the voltameter now yielded one cubic inch in forty seconds: two-thirds of the wire immersed gave one cubic inch in thirty-seven seconds; and five-sixths immersed gave one cubic inch in thirty-five seconds. The heat of the portion of wire not immersed in water had, in the last experiment, nearly reached the point of fusion of the platinum. By this result it appears that the increased resistance to conduction of the ignited portion is not equal to the increased conducting power of the cooled portion of the same wire.

With a view of seeing how far the cooling effect upon the ignited wire might be due to the greater or less fluency or mobility of the particles of the different media surrounding it, I have looked into the papers of Faraday * and of Graham. † In the experiments of the former it appears that the escape of different gases at a certain pressure through capillary tubes, or the velocities of revolution of vanes or floats surrounded by different gases, was in some inverse ratio to the density of

* *Quarterly Journal of Science*, vol. iii., p. 354.
† *Phil. Trans.*, 1846, p. 573.

such gases; and the experiments of the latter show that the effusion or escape of gases through a minute aperture in a plate takes place with velocities inversely as the square root of their specific gravities. In Graham's experiments, however, when the escape took place through capillary tubes, the results seemed subject to no ascertained law, though the compounds of carbon with hydrogen passed through with greater facility than other gases.

The cooling effects of gases on the ignited wire are decidedly not in any ratio with their specific gravities; thus, carbonic acid, on the one hand, and hydrogen, on the other, produce greater cooling effects than atmospheric air; and olefiant gas, which closely approximates air, and is far removed from hydrogen in specific gravity, much more nearly approximates hydrogen, and is far removed from air in its cooling effect.

Upon the whole we may conclude, from the experiments detailed in this paper, that the cooling effect of different gases, or rather the difference in the cooling effect of hydrogen and its compounds from that of other gases, is not due to differences of specific heat; it is not due to differences of specific gravity; it is not due to differences of conducting powers for electricity; it is not due to the character of hydrogen in relation to its transmission of sound, noticed by Leslie, for reasons which I have before given;* it is not due to the same physical characters of mobility which occasion one gas to escape from a small aperture with greater facility than another; but it may be, and probably is, affected by the mobile or vibratory character of the particles by which heat is more rapidly abstracted. I at one time thought that the effect might have relation to the combustible character of the gas, and that the electro-negative gases were in respect to it contra-distinguished from the electro-positive or neutral gases, but the experience I have obtained from the experiments detailed here induces me to abandon that supposition.

I incline to think that, although influenced by the fluency of the gas, the phenomenon is mainly due to a molecular action

* *Phil. Trans.*, 1847.

at the surfaces of the ignited body and of the gas. We know that in the recognised effects of radiant heat the physical state of the surface of the radiating or absorbing body exercises a most important influence on the relative velocities of radiation or absorption; thus, black and white surfaces are, as every one knows, strikingly contra-distinguished in this respect: why may not the surface of the gaseous medium contiguous to the radiating substance exercise a reciprocal influence? Why may not the surface of hydrogen be as black and that of nitrogen as white to the ignited wire? This notion seems to me the more worthy of consideration, as it may establish a link of continuity between the cooling effects of different gaseous media and the mysterious effects of surface in catalytic combinations and decompositions by solids, such as platinum. Epipolic actions will, I feel convinced, gradually assume a much more important place in physics than they have hitherto done; and the farther development of them appears to me the most probable guide to the connection by definite conceptions of physical and chemical actions.*

The difference of the cooling effect of hydrogen, and of those of its compounds, where it is not neutralised by a powerful electro-negative gas, from all other gases, is perhaps the most striking peculiarity of the phenomena I have described. The differences of effect of all gases other than hydrogen and such compounds are quite insignificant when compared with the differences between the hydrogenous and the other gases. There are some phenomena which I have before observed, and which were, at the time I noticed them, inexplicable to me; but they now appear dependent on this physical peculiarity of hydrogen. Thus, if a jet of oxygen gas be kindled in an atmosphere of carburetted hydrogen, the flame is smaller than when the converse effect takes place. The voltaic arc between metallic terminals is also much smaller in hydrogen gas than in nitrogen, though both these gases are incapable of combining with the terminals; indeed, to obtain an arc at all in hydrogen is scarcely practicable.

Davy has, in his 'Researches on Flame,' given several

* See Dr. Tyndall's 'Researches on the Absorption and Radiation of Heat by Gaseous Matter.'—*Phil. Trans.*, 1861, 1862, and 1864.—W.R.G., 1874.

experiments which are similarly explicable; but, though noting the results, he nowhere, as far as I am aware, attributes them to any specific peculiarity of hydrogen.

Of the phenomenon which I have examined in this paper I first published an account in connection with some experiments on the application of voltaic ignition to lighting mines, and it does not appear impossible that the experiments now detailed may ultimately find some beneficial application in solving the problem of a safety-light for mines. A light which is just able to support itself under the cooling effect of ordinary atmospheric air would be extinguished by air mixed with hydrogenous gas.

I am far from pretending to have devised any means of fulfilling these conditions, and yet supplying an efficient light; I merely throw it out as a suggestion for consideration, knowing that there are no additions to our knowledge which are not ultimately valuable in their practical application; and that a suggestion, however vague—a new point to those whose minds may be occupied with the subject—may lead them to results which he who makes the suggestion is unable to attain.

P.S.—Since this paper was communicated I have received a paper from Dr. Andrews, of Belfast, who published as early as 1840, in the 'Proceedings of the Royal Irish Academy,' experiments similar to those of mine first published in 1845. My experiments were made in the same year as those of Dr. Andrews, but as I withheld their publication, Dr. Andrews is fully entitled to priority. Had I known of his experiments earlier, I should have recited them in the first part of this paper.

ON THE PROGRESS MADE IN THE APPLICATION OF ELECTRICITY AS A MOTIVE POWER.

Royal Institution, February 9. *Literary Gazette*, February 17, 1844.

THE object of this lecture was to review the various applications of the electro-magnetic force which have been made since

its first discovery by Oersted, in 1819, and first application to actual rotary motion by Faraday. After explaining and illustrating by many novel experiments the character and direction of the force exercised by voltaic currents upon magnets, Mr. Grove pointed out three definite ramifications of the force which different experimentalists had sought to apply to the practical production of mechanical motion: 1. The immediate tangential or deflective power of the current, which is shown in the ordinary galvanometer, where a magnet is deflected by a stationary wire affected by a voltaic current, and in the revolving wheel of Barlow, where the radii are deflected and revolve by the influence of a stationary magnet. 2. What is called the suspension principle, where powerful stationary electro-magnets are made to attract pieces of soft iron placed in the periphery of a wheel. As each piece arrives opposite the magnet, the voltaic current is interrupted by the revolution of the wheel itself, and this passes on until the next piece comes within the influence of the magnet, which, being re-electrised, again attracts, and so on in rotation. 3. The principle of inversion of polarity, first introduced by Dr. Ritchie. In this two sets of magnets are employed, the one set stationary, and the other rotary; the poles of the last being alternately changed by inversion of the voltaic current, the attractions of the two opposite poles of powerful magnets are rendered available.

Beautiful working models of the application of the several principles were exhibited, most of them representing patented machinery.

Mr. Grove then entered upon the statistics of expense of electro-magnetic machines. It appears from the experiments of Dr. Botto, of Turin, that the consumption of 45 lbs. of zinc is necessary to work a one-horse power magnetic machine for twenty-four hours. Dr. Botto worked with Mr. Grove's battery, against which, although admitted to be by far the most powerful and equal in constancy to any other form, a general notion prevails that it is expensive. This Mr. Grove says is not the case, and gave his reasons, as follows:—To contrast the current cost of the nitric acid or Grove's battery with the apparently cheapest form—namely, a battery charged with

dilute sulphuric acid only—this latter, to overcome a given resistance, say to decompose an equivalent of water, requires a series of at least three cells, while only *one* of the nitric acid battery is requisite. Hence we have in the first, three equivalents of zinc and three of sulphuric acid consumed; in the second, one of zinc, one of sulphuric acid, and one-third of nitric acid; for nitric acid having five equivalents of oxygen, three of which are actually available, does three units of work. The calculation then becomes as given in the annexed table:—

Chemical equivalent of zinc, 32.
Chemical equivalent of nitric acid, 54.
$54 \div 3 = 18$.
$32 : 18 :: 45 : 25{\cdot}3$.
45 lbs. of zinc at $3d. = 11s.\ 3d.$
25·3 lbs. of real (*i.e.* 50·6 of commercial) nitric acid, at $6d. = 1l.\ 5s.\ 7\frac{1}{2}d.$
$11s.\ 3d. + 1l.\ 5s.\ 7\frac{1}{2}d. = 1l.\ 16s.\ 10\frac{1}{2}d.$, the expense of one horse-power for twenty-four hours.

In this the small expense of the sulphuric acid and minute waste of mercury are thrown out of the equation, as more than balanced by the salts formed during the action of the battery. It is evident from this that the expense of electro-magnetic machines far exceeds that of steam; indeed, it could hardly be expected to be otherwise, as with the one we use for fuel manufactured materials, in the production of which coals, labour, &c., have been expended; in the other, coals and water are used directly. This is rather discouraging; but at the same time we must recollect that in magnetic motion nothing is wasted; when the machine stops the consumption stops also; and Mr. Grove thinks that we should not so much direct our endeavours to rival steam, as to find out methods of application where steam cannot be applied. The telegraph is one of these, and probably some others will be found; at all events, the idea that so much ingenuity, labour, and perseverance should have been applied, and should have marched with rapid strides in vain, is contradicted by the whole history of science; and though it may not be very easy to point out the exact where, when, and how of its success, still,

considering the enormous progress of electro-magnetic applications made within the last few years, there is every ground for hopefulness as to its ultimate success.

In a lecture at the Royal Institution, in 1849,* Mr. Grove discussed certain practical applications of voltaic ignition and the voltaic arc, such as eudiometry, lighting mines, street-lighting, and light-houses. He had made some experiments six years ago on the subject, and then on one occasion delivered a lecture at the London Institution, the theatre being illuminated by the voltaic arc. In preparing the lecture he had made a rough calculation as to expense, and the matter appeared to him (though attended with many practical difficulties) to be hopeful and promising. By interposing a voltameter in the circuit while the arc was produced the consumption in the battery could be calculated; for every chemical equivalent of hydrogen evolved in the voltameter an equivalent of zinc, of sulphuric acid, and one-third of an equivalent of nitric acid, would be consumed in each cell of the battery. Supplying these data for calculation, and making proper allowance for the amount of water contained in the commercial acids, &c., the theoretical expense of a battery such as he was exhibiting (fifty cells of the nitric acid combination, each platinum plate two inches by four) would be about two shillings an hour. He had tested by the photometric method of equality of shadows the intensity of the light as compared with a common wax-candle, and found that after the battery had been an hour at work the voltaic light was to the candle as 1,444 to 1. He did not take this comparison of intensities as an absolutely fair practical comparison, nor did he give the above as a practical calculation, but thought it would be safe if twice that expense, or four shillings per hour, were assumed; the actual expense of charging the battery for a given time of action bore this out. He showed the inferiority of central as compared with separate lights for street illumination, but for lighthouses, particularly for an intermittent light at regular intervals, or for signal lights, the application appeared to him to be reasonably approximate, and for more general purposes far from hopeless;

* *Lit. Gaz.*, Feb. 24, 1849.

the practical difficulties, though undoubtedly not small, being, in his opinion, by no means insurmountable.

Electrical Mag., p. 601. Jan. 1845.

Mr. Grove communicated to the proprietors of the London Institution, at this their first soirée for the season, some of the leading discoveries in physical science during the past year.

Of electrical subjects, M. Matteucci's researches were described, with experimental illustrations; as also the magnetic note. In reference to the latter Mr. Grove detailed a curious experiment that had occurred to him, and which bore greatly on the subject. A glass tube, with plate glass at the ends, and protected along its length with a copper jacket, was filled with water, in which was suspended powdered magnetic oxide of iron. On looking through the tube at distant objects a considerable portion of the light was intercepted by the heterogeneous arrangement of the particles of the oxide; but, on passing a current through a coil placed round the tube, these particles assumed a symmetrical character, and much more light was transmitted. This experiment was alluded to as an illustration of the molecular polarisation and vibration that is attributed to the particles of an iron rod, when a musical note is obtained under the influence of the current.

[This result was the small outcome of a series of experiments which ought to have led me to the discovery afterwards made by Faraday. At the time I made the above experiment I tried a tube having a coil of coated wire round it, and a Nichol's prism at each end. The tube was filled successively with various solutions, and I hoped to divert the plane of polarisation by passing a voltaic current through the coil. I failed only because my coil was a single one, and not several times reduplicated, as it should have been. I mentioned the experiment at the time, to Mr. Gassiot, Faraday, and Pereira, and the latter communicated it to the Managers of the London Institution. I subsequently observed that a plate of mica in vacuo placed itself transversely to the line joining the poles of a powerful electro-magnet, and was engaged on this when Faraday published his paper on the magnetisation of all matter, which included and explained the single case I was working at.]—W.R.G., 1874.

ON THE DIRECT PRODUCTION OF HEAT BY MAGNETISM.

Proc. R. S. May 24, 1849.

THE author recites the experiments of Messrs. Marrian, Beatson, Wertheim, and De la Rive on the phenomenon made known some years ago, that soft iron when magnetised emitted a sound or musical note.

He also mentions an experiment of his own, published in January 1845, where a tube was filled with the liquid in which magnetic oxide had been prepared, and surrounded by a coil; this showed to a spectator looking through it a flash of light when the coil was electrised.

These experiments led to the supposition that when iron is magnetised a molecular change is produced throughout its mass; if this be the case, a species of molecular friction might be expected to obtain, and by such molecular friction heat might be produced.

Difficulties present themselves in proving this result, the principal of which is that with electro-magnets the heat produced by the electrised coil surrounding them, might be expected to mask any heat developed by the magnetism.

This interference, after several experiments, the author considers he entirely eliminated by surrounding the poles of an electro-magnet with cisterns of water, and by this means, and by covering the keeper with flannel and other expedients, he was enabled to produce in a cylindrical soft iron keeper, when rapidly magnetised and demagnetised in opposite directions, a rise of temperature several degrees beyond that which obtained in the electro-magnet, and which therefore could not have been due to conduction or radiation of heat from such magnet. A series of experiments is given with this apparatus.

By filling the cisterns with water colder than the electro-magnet, the latter could be cooled by the water while the keeper was being heated by the magnetisation.

The author subsequently obtained distinct thermic effects in a bar of soft iron placed opposite to a rotating permanent

steel magnet, using a delicate thermo-electrical apparatus placed at his disposal by Mr. Gassiot.

To eliminate the effects of magneto-electrical currents, the author then made similar experiments with non-magnetic metals and with silico-borate of lead, substituted for the iron keepers, but no thermic effects were developed.

He then tried the magnetic metals nickel and cobalt, and obtained thermic effects with both, and in proportion to their magnetic intensity.

Some questions of theory suggested by the above experiments, and relating to the *rationale* of the action of what are termed 'the imponderables' and to terrestrial magnetism, are then briefly discussed, and the author concludes by stating that he considers his experiments prove satisfactorily that whenever a bar of iron or other magnetic metal is magnetised, its temperature is raised.

ON THE ELECTRO-CHEMICAL POLARITY OF GASES.

Phil. Trans. R. S. Received January 7. Read April 1, 1852.

THE different effects of electricity upon gases and liquids has long been a subject of interest to physical enquirers. There are, as far as I am aware, no experiments which show any analogy in the electrisation of gases to those effects now commonly comprehended under the term electrolysis. Whether gases at all conduct electricity, properly speaking, or whether its transmission is not always by the disruptive discharge, the discharge by convection, or something closely analogous, is perhaps a doubtful question,* but I feel strongly convinced that gases do not conduct in any similar manner to metals or electrolytes.

In a paper published in the year 1849† I have shown that

* See M. Edmond Becquerel's experiments and one by myself by which I detected a slight transmission of electricity between two highly ignited platinum wires in close proximity and in the vacuum of a good air-pump.—*Notices, R. I.*, Feb. 3, 1854. See note to p. 344.

† *Phil. Trans.*, 1849, p. 55.

hydrogen or atmospheric air intensely heated showed no sign of conduction for voltaic electricity, even when a battery of very high intensity was employed.

In the Eleventh, Twelfth, and Thirteenth Series of Faraday's Experimental Researches the line of demarcation between induction across a dielectric and electrolytic discharge is repeatedly adverted to ; induction is regarded as an action of contiguous particles, and as a state of polarisation anterior to discharge, whether disruptive, as in the case of dielectrics, or electrolytic, as in electrolytes. See §§ 1164, 1298, 1345, 1368, &c.

Mr. Gassiot, in a paper published in the year 1844,* has shown that the static effects, or effects of tension, produced by a voltaic battery, are in some direct ratio with the chemical energies of the substances of which the battery is composed ; in other words, that in a voltaic series, whatever increases the decomposing power of the battery when the terminals are united by an electrolyte, also increases the effects of tension produced by it, when its terminals are separated by a dielectric.

In none of the above papers, and in no researches on electricity of which I am aware, is there any experimental evidence that the polarisation of the dielectric is or may be chemical in its nature ; that, assuming a dielectric to consist of two substances having antagonistic chemical relations, as for instance oxygen and hydrogen, the particles of the oxygen would be determined in one direction, and those of the hydrogen in the other ; the only experimental result bearing on this point with which I am acquainted is the curious fact which was observed by Mr. Gassiot and some other electricians who experimented with him in the year 1838, viz. that when two wires forming the terminals of a powerful battery were placed across each other, and the voltaic arc taken between them, the extremity of the wire proceeding from the positive end of the battery was rendered incandescent, while the negative wire remained comparatively cool ; it was at that time believed that there was some effect exhibited here *extra* the voltaic

* *Phil. Trans.*, 1844, p. 39.

circuit. Shortly afterwards I showed that with all, or at all events a great number of metals, the positive terminal was more heated than the negative, and that the portion of the crossed wire which was positive became more incandescent than that of the negative, from the greater heating effect developed at the positive point when the disruptive discharge took place. I suggested as an explanation of this phenomenon the possibility that in air, as in water, or other electrolyte, the oxygen or electro-negative element was determined to the positive terminal, and that from the union of the metal with that of oxygen a greater heating effect was developed. This, with some other impressions, which turned out to be erroneous, I mentioned in a letter to my friend Dr. Schönbein, not intended for publication, but which shortly afterwards found its way into print.*

Though by no means thinking that this explanation was in every respect satisfactory, there were many arguments in its favour, and the fact strongly impressed my mind as evincing a very striking difference in character between the effect of the discharge at the positive and negative terminals, and as presenting, as far as it went, a distant analogy to the effect of electrolysis.

In the year 1848, while experimenting with Mr. Gassiot with a nitric acid battery consisting of 500 well-insulated cells, I made the following experiment: Two wires of platinum $\frac{1}{40}$th of an inch in diameter, forming the terminals of the battery, were immersed in distilled water; the negative wire was then gradually withdrawn until it reached a point a quarter of an inch distant from the surface of the water. A cone of blue flame was now perceptible, the water forming its base, and the point of the wire its apex; the wire rapidly fused, and became so brilliant that the cone of flame could be no longer perceived, and the globule of fused platinum was apparently suspended in air and hanging from the wire; it appeared sustained by a repulsive action like a cork ball on a *jet d'eau*, and threw out scintillations in a direction away from the water. The surface of the water at the base of the cone was depressed, and divided into little concave cups, which were in a

* *Phil. Mag.*, 1840, p. 478.

state of continual agitation. When the conditions were reversed and the negative wire immersed, the positive wire being at the surface, similar phenomena ensued, but not nearly in so marked a manner; the cone was smaller, and its base much more narrow in proportion to its height.

This experiment, the beautiful effect of which requires to be seen to be appreciated, indicates a new mode of transmission of electricity partaking of the electrolytic and disruptive discharges. Not possessing a battery of this enormous intensity, I have not been able to examine this phenomenon more in detail; but I have from time to time made many other experiments on the voltaic arc taken in various gaseous media, with the view of ascertaining the state of the intervening media anterior to, during, and after the discharge; these experiments have hitherto given me no results of any value. In the voltaic arc the intense heat developed so affects the terminals and so masks the proper electrical effect, that the difficulty of isolating the latter is extreme; and I have latterly sought for some modified form of electric discharge which should be intermediate between the voltaic arc and the ordinary Franklinic discharge or that from the prime conductor of a frictional machine; for something, in short, which should yield greater quantitative effects than the electrical machine, but not dissipate the terminals, as is done by the voltaic arc.

An apparatus, to which M. Despretz was kind enough to call my attention recently at Paris, seemed to promise me some aid in this respect. It was constructed by M. Ruhmkorff, on the ordinary plan for producing an induced current, viz. a coil of stout wire round a soft iron core, with a secondary coil of fine wire exterior to it, having an ingenious self-working contact breaker attached; from the attention paid to insulation in the construction of this apparatus very exalted effects of induction could be procured. Thus, in air rarefied by the air-pump an aurora or discharge of five or six inches long could be obtained from the secondary coil, and in air of ordinary density a spark of one-eighth of an inch long.

I procured one of these apparatus from M. Ruhmkorff; the size of the coil portion of the apparatus is 6·5 inches long,

4 inches diameter; the length of the wires forming the coils are (I give M. Ruhmkorff's measurements), stout wire, 30 metres long, 2 millimetres diameter, 200 convolutions; fine wire, 2,500 metres long, $\frac{1}{4}$ millimetre diameter, 10,000 convolutions. These measurements will only be taken as approximative, and indeed the exact size is immaterial to the consideration of the experiments which I am about to detail. I will not give my experiments in the order in which I made them, as I should have to describe many fruitless ones, but I will place first that which I consider the most important and fundamental.

1st. On the plate of a good air-pump was placed a silvered copper plate, such as is ordinarily used for Daguerreotypes, the polished silver surface being uppermost. A receiver, with a rod passing through a collar of leathers, was used, and to the lower extremity of this rod was affixed a steel needle, which could thus be brought to any required distance from the silver surface; a vessel containing potassa fusa was suspended in the receiver, and a bladder of hydrogen gas was attached to a stop-cock, another orifice enabling me to pass atmospheric air into the receiver in such quantities as might be required.* A vacuum being made, hydrogen gas and air were allowed to enter the receiver in very small quantities, so as to form an attenuated atmosphere of the mixed gas. There was no barometer attached to my air-pump, but from separate experiments I found the most efficient extent of rarefaction for my purpose was that indicated by a barometric height of from half to three-quarters of an inch of mercury; and, except where otherwise stated, a similarly attenuated medium was employed for all the following experiments.

Two small cells of the nitric acid battery, each plate exposing four square inches of surface, were used to excite the coil machine, and the discharge from the secondary coil was taken between the steel point and the silver plate. The distance between these was generally $= 0{\cdot}1$ of an inch, but this may be considerably varied. When the plate formed the positive terminal, a dark circular stain of oxide rapidly formed

* See a figure and description of the apparatus at the end of this Paper.

on the silver, presenting in succession yellow, orange, and blue tints, very similar to the successive tints given by iodising in the ordinary manner a Daguerreotype plate. Upon the poles being reversed and the plate made negative, this spot was entirely removed, and the plate became perfectly clean, leaving, however, a dark, polished spot occasioned by molecular disintegration, and therefore distinguishable from the remainder of the plate.

The experiment was repeated a great many times, and with varying proportions of gas, and I found that with proportions varying from equal volumes of hydrogen and air to those of one volume of the former to two and a half of the latter, the experiments succeeded ; better, I should say, when there was rather an excess of hydrogen as compared with the equivalent of oxygen in the atmospheric air ; about one volume of hydrogen to one and a half of air succeeded well ; when excess of air was present, oxidation took place, whether the plate was positive or negative, and when excess of hydrogen was present no oxidation took place.

2nd. I experimented with an air vacuum (to borrow an expression of Dr. Faraday), and found that oxidation took place whether the plates were positive or negative, but in different degrees ; when the plate was positive, a small circular spot was rapidly formed, quickly deepening in colour, and apparently eating into the plate ; when the plate was negative, a large diffuse spot was formed, the oxidation was more slow, and the plate not so rapidly corroded.

3rd. I now operated with a hydrogen vacuum ; when the plate was clean no discolouration took place, the plate retained its polish, though after a long continuance of the discharge a molecular change was perceptible, producing a frosted appearance similar to the mercurialised portions of a Daguerreotype.

When the plate had been previously oxidated by the discharge in an air vacuum the oxidation was rapidly and beautifully cleared off by the discharge in the hydrogen vacuum, and this whether the plate was positive or negative, the effect being, however, better and more rapidly produced in the latter case.

4th. I substituted respectively for the steel needle wires

of copper, silver, and platinum, and found the effect produced by all and with nearly equal facility; if there were any difference, the platinum point was the least efficient; this may be due to the peculiar effect of platinum in itself combining the gases, or to its inoxidable character, the oxygen being thrown off from its surface, and not uniting with it, as with the more oxidable metals; the flame or luminous appearance which surrounded the wire when the platinum was negative was larger and more diffuse than with the other metals.

5th. As air, notwithstanding its containing a great excess of nitrogen, gave an effect of oxidation at both electrodes, though different in degree, I increased the proportion of nitrogen by passing into the receiver nitrogen which had been formed by the slow combustion of phosphorus, the phosphorous acid having been well washed away, and potash being always in the receiver; no more air was allowed to be present than the very small quantity released from the apertures of the stopcock; with this mixture, viz. a maximum of nitrogen and a minimum of oxygen, and rarefied as before, a similar effect was produced to that shown in the mixture of air and hydrogen, the positive plate being oxidated by the discharge, and the spot when made negative being reduced. The effect of reduction was not so rapid, or so readily produced, as when hydrogen was used, but was very decided.

6th. With nitrogen, as much deprived of oxygen as I could procure, the colours of oxidation were not exhibited, but a dark spot, apparently due to disintegration, was produced, which was not removed by the plate being made negative; if, however, the coloured spot was produced by the plate being made positive in an air vacuum, they were removed by the plate being made negative in a nitrogen vacuum, leaving, however, a darker spot than that which was exhibited when they were reduced in hydrogen. Even when produced in an air vacuum, and then a very perfect exhaustion effected, such as would reduce the mercury in the barometer to the height of $\frac{1}{20}$th of an inch, the spot was partially reduced when the plate was made negative.

7th. An oxyhydrogen vacuum was formed, the gases being in the proportion in which they form water; and, thanks to

the attenuated atmosphere, it was easy to take the discharge in this mixture without producing detonation or any sudden combination of the gases, a possibility pointed out by Grotthus.* With this mixture the effect took place as with the mixture of atmospheric air and hydrogen. I expected it to have been more efficient, but it was rather less so than the mixture of air and hydrogen; whether it be that the presence of nitrogen lessens the tendency to combine of the gases oxygen and hydrogen, and thus enables the electrical polarisation and discharge to operate more efficiently, whether the nitrogen has a specific effect in aiding the electro-chemical effect, as I have shown it has in one peculiar case, † or whether any unknown effect of nitrogen is concerned, I do not undertake to pronounce; I can only say that in several repetitions of the experiment it appeared to me that the mixture of atmospheric air and hydrogen was more efficient in exhibiting this phenomenon that that of oxygen and hydrogen.

8th. Different proportions of oxygen and hydrogen were employed, and here also I found that within a tolerably wide margin I could vary the proportion of the gases. Three volumes of hydrogen to one volume of oxygen I found to be a very efficient mixture.

9th. I now substituted for the silver plate plates of the following metals: bismuth, lead, tin, zinc, copper, iron, and platinum, the former three metals being burnished, the latter polished.

Bismuth showed the effect nearly, if not quite as well as silver; it was oxidated in an air vacuum, reduced in a hydrogen vacuum, and oxidated or reduced in the mixed gas, according as it formed the positive or negative terminal.

Lead oxidated easily, but the spot of oxide could with difficulty be reduced. Tin, zinc, and copper required the admission of a great quantity of air to produce oxidation; and I could not succeed in reducing the oxide by the electrical discharge, at least so as to restore the polish of the plate; a blackening effect was in some degree produced. Iron was not oxidated until the receiver was nearly filled with air, and then

* *Annales de Chimie*, vol. lxxxii. † *Phil. Trans.*, 1843, pp. 110, 111.

a small spot of rust was formed, which I could not reduce. With all the metals a slight whitish film, like the mercurialised portion of a Daguerreotype, was visible beyond the circle marked by the discharge when the plate was rendered positive, which film was removed by negative electrisation in a hydrogen vacuum; it seemed to me that this film, as well as others among those I have described, was affected by light, but I did not turn aside to examine this effect. Platinum showed no effect either of oxidation or reduction.

10th. As it was impossible to operate with an atmosphere of chlorine with the apparatus which I possessed, and wishing to vary the electro-negative element, I iodised a silver plate by the vapour of iodine to a deep blue colour, and then made it negative in an atmosphere of hydrogen: the iodine was beautifully removed in a circle or disc opposite the point which formed the positive terminal.

11th. I now substituted for the coil apparatus a very good electrical machine, the cylinder of which was 16 inches diameter, and the prime conductor of which, when the machine was properly excited, gave a spark of 8 inches long. With this machine, and in an attenuated atmosphere of one volume hydrogen plus two of atmospheric air, I produced the effects of oxidation and reduction very distinctly, the plate being in turn connected with the conductor and with the ground; but the comparative minuteness of the spot, after many turns of the machine, showed the great superiority of the coil machine for producing quantitative effects over the ordinary electrical machine; and I question whether I should have detected the phenomenon with the latter, had I not become previously well acquainted with it by the former apparatus. Probably an extensive series of the water battery or a steam hydro-electric machine would succeed equally well or better than the coil machine.

12th. A solution of hyposulphite of soda removed the spots formed by electrisation from the silver plate, just as it removes the iodine from an iodised plate.

13th. In some of the above experiments I remarked a tendency in the spots produced by the discharge to show circles or zones of oxidation in different degrees, and in a more

marked manner than would be accounted for by the different colours of the thin films of oxide formed. I determined to examine this effect, and selected, after some experiments, an atmosphere of one volume oxygen mixed with four volumes of hydrogen, and attenuated by the air-pump, as in the previous experiments. The plate was made positive, and the point was placed successively opposite different portions of the silver plate, at distances of $\frac{1}{50}$th, $\frac{2}{50}$ths, $\frac{3}{50}$ths, $\frac{4}{50}$ths, and $\frac{5}{50}$ths of an inch. The results are given, as nearly as I can copy them, in the accompanying plate, figs. 1 to 5.

The colour of the central spot was a yellow-green in the centre, surrounded by a blue-green, then a clear ring of polished silver, then an outer ring of crimson, with a slightly orange tint on the inner side, and deep purple on the outer; the exterior portion of the spot was, as far as my eye could judge, of a colour complementary to the interior of the external ring, and the central portion of the spot of a colour complementary to the exterior portion of the ring. The colours varied with the time, density of gas and other conditions, but generally showed this complementary tendency. Symptoms of a faint polished ring were visible beyond the outer ring, and could be rendered more distinct by breathing on the plate. As the distance between the point and the plate was increased the colours became fainter, and the rings more diffuse, and beyond the distance I have given nearly lost their defined character; but the first three distances, or those of $\frac{1}{50}$th, $\frac{2}{50}$ths, and $\frac{3}{50}$ths of an inch gave very beautifully defined rings. The luminous appearance on the needle in these experiments extended from three-fourths of an inch to an inch from the point. Frequently a small polished speck was visible, exactly opposite the point of the needle. See fig. 6.

When the plate was made negative, the other conditions being the same, a polished space appeared opposite the point of the needle, surrounded by a dusky and ill-defined areola; its colour, when regarded from a point opposite the incident light, was brown tinged with purple; and when in the same direction as the light, a greenish white, similar to the tint seen on mildew or on some of the lichens: these spots were very different from the positive spots, and in some degree the

converse of them; but they were not nearly so well defined or capable of being produced with the same uniformity. I have endeavoured to represent one of them at fig. 7.

14th. In order to ascertain whether the polished ring intervening between the oxidated central spot and oxidated external ring were a mere negation of effect or an antithetic polar effect, such as would occasion reduction, I formed in an air vacuum two large spots on a silver plate, with one the plate being made negative, and with the other positive, oxidating them until they began to pass from deep orange to purple. I then perfectly exhausted the receiver, swept it with the gas employed in the last experiment, and then took the discharge in a vacuum of that gas, viz. one volume oxygen + four hydrogen; the plate being positive, and the needle $\frac{2}{50}$ths of an inch over the centre of each spot in turn, a ring of clear polish was formed rapidly in both the dark discs, just at the distance where the ring of polish appeared in the last experiment. I then exposed a clean portion of the plate to the needle without any other change, and, on allowing the discharges to pass, formed the rings, just as in the last experiment.

15th. I examined some of the spots with an achromatic microscope, magnifying 200 diameters; I could not, however, discover any feature which the naked eye did not show, or any peculiar molecular state; the polishing scratches on the plate were highly magnified, but the electrised spots only showed more dimly the colours or the lights and shadows which they exhibited to the naked eye.

16th. I took the discharge on a silver plate in vacua of the following gases respectively: Oxygen, protoxide of nitrogen, deutoxide of nitrogen, carbonic acid, carbonic oxide, and olefiant gas.

The first four gases presented nothing remarkable; the plate was oxidated, whether positive or negative, as in a vacuum of atmospheric air. In the protoxide of nitrogen the colour of the discharge was a beautiful crimson on both terminals.

In deutoxide of nitrogen a greater tendency to reduction was shown when the plate was negative than in the other three gases, and there was also a tendency to the formation of rings. In carbonic oxide the plate was oxidated when positive,

and the oxide reduced when negative, just as with a vacuum of air and hydrogen, but rather more slowly; with a mixture of five volumes of carbonic oxide and one volume of oxygen, the rings were formed very distinctly, particularly if the plate was made negative first, and then positive. The luminous spot on the plate, when positive in this gas, was coloured green.

When the plate was negative in olefiant gas it darkened, showing the rings of colour produced by thin plates, and very distinct from the other rings of which I have spoken. After a short time a pulverulent deposit was formed on the plate, giving brilliant sparks or stars of light, which were not shown by any other gas.

This deposit was too minute for analysis, but I have no doubt, from the gas used and the appearances presented, it was carbon.

I have given in the above experiments the conditions under which they succeeded best; but, upon repetition, although the exact volumes of gases and other conditions were carefully attended to, they sometimes required a slight alteration to succeed, variations taking place from causes which I could not detect; thus it was sometimes necessary to add a little more hydrogen, sometimes a little more oxygen or air, to alter slightly the state of attenuation in the gas, &c.

The necessarily varying condition of the battery, and the state of the contact breaker, slight impurities in the gases or on the surface of the plates, would be quite sufficient to account for these irregularities. I mention them for the guidance of anyone who may wish to repeat the experiments; a very little practice will enable any electrician to have the results at his command. When there is too great a proportion of air or oxygen, oxidation takes place at both poles; when too much hydrogen, reduction takes place at both; and to effect oxidation or reduction by reversing the direction of the discharge, an intermediate condition is requisite; so if the gas be not sufficiently attenuated the oxidation is too rapid, and the plate too much corroded to bring out the effects clearly; if too much attenuated too long a time is required, and the effect is feeble and indistinct.

I have above selected all the experiments which I consider material in this, I believe, new class of phenomena. The spots produced by electrical discharges, both on conducting bodies and on electrics, have been before noticed and experimented on, one class by Priestley,* and another class by Karsten† and others, but as far as I am aware no distinct electro-chemical action in dry gases, depending upon the antithetic state of the terminals, and presenting a definite relation of the chemical to the electrical actions in gaseous media, has been pointed out. I now proceed to consider the relation which these results bear to other electrical phenomena.

As may be gathered from my opening remarks, the experiments above detailed appear to me to furnish a previously deficient link in the chain of analogy connecting dielectric induction with electrolysis. The only satisfactory *rationale* which I can present to my own mind of these phenomena is the following: The discharges being interrupted (as is evident from the nature of the apparatus, and may be easily proved by agitating a mirror near them and regarding their reflected images in the moving mirror), the gaseous medium is polarised anterior to each discharge, and polarised not merely physically, as is generally admitted, but chemically, the oxygen or anion being determined to the positive terminal or anode, and the hydrogen or cation being determined to the negative terminal or cathode; at the instant preceding discharge there would then be a molecule or superficial layer of oxygen or of electro-negative molecules in contact with the anode, and a similar layer of hydrogen or of electro-positive molecules in contact with the cathode; in other words, the electrodes in gas would be polarised, as the electrodes in liquid are. The discharge now takes place, by which the superficial termini of metal or of oxide, as the case may be, are highly ignited or brought into a state of chemical exaltation at which their affinities can act; the anode thus becomes oxidated, and the cathode, if an oxide, reduced. I have elsewhere‡ shown strong reasons for

* *History of Electricity*, 2nd edit., p. 624.

† *Archives de l'Electricité*, vol. ii. p. 647; vol. iii. p. 310.

‡ *Phil. Trans.*, 1847, pp. 10, 16, 21. *Correlation of Physical Forces*, p. 50, 2nd edit.

assuming that the electric or voltaic discharge, the moment polarity is subverted, may be regarded as an intensely heated state of the particles of the electrodes, and of the intermedium across which it passes; and my present explanation is perfectly consistent with and derivable from my previous views of the disruptive discharge.

Two other theories might be proposed to account for the phenomena I am considering; the one, that the disruptive discharge itself is analogous to the electrolytic, and that the oxygen and hydrogen are reciprocally transferred by the discharge itself. This would not, I think, be consistent with the generally known facts connected with the discharge, and is entirely ineffectual in explaining the Experiments 2nd and 3rd, where either the positive or negative terminal can be made either to oxidate or reduce, according to the nature of the chemical medium present, while these experiments are entirely in accordance with, and the results of them flow as a necessary consequence of, the view first advanced. The other theory which may be advanced is, that by dielectric induction the gases may be bodily separated, a layer, not molecular, but corporeal or voluminous, if I may be allowed these expressions, of oxygen being developed on the side next the anode, and one of hydrogen next the cathode, the gas intervening between the terminals being thus divided, as it were, into two halves: this would certainly be a most curious phenomenon, but I believe it to be so inconsistent with the vast mass of accumulated facts in electrical science, and likely to have produced in cosmical phenomena so many results which, if existing, must long ere this have been detected, that I will not do more than advert to it.

I have adopted the view which I have first stated as being the least removed from ordinary theories or modes of regarding electrical phenomena, and because in the present instance I can present the phenomena in no other way which is in the least degree satisfactory to my own mind, while this view to me well accounts for them. Assuming, then, for the present this view, we get a close approximation, I may say an identity of the state of polarisation in gaseous non-conducting dielec-

trics, and in electrolytes anterior respectively to discharge or to electrolysis.

Faraday observes, 'Experimental Researches,' 1164, 'In an electrolyte induction is the first state, and decomposition the second.' My present experiments show, I believe, that in induction across gaseous dielectrics there is a commencement, so to speak, of decomposition, a polar arrangement not merely of the molecules, irrespective of their chemical characters, but a chemical alternation of their forces, the electro-negative element being determined or directed, though *not travelling* in one direction, and the electro-positive in the opposite direction.

This arrangement is only evidenced at present, as it is in electrolysis, by the action at the polar extremities or termini of the dielectric; possibly future researches may show, by the action of polarised light, by magnetism or some other means of analysis, that the polarity extends, as we theoretically believe it does, through the whole intervening matter.

In the Experiment No. 5, with oxygen and excess of nitrogen, reduction takes place by the effect of negative electricity and heat; at least there seems every reason from analogy to believe that the effect of the nitrogen is only negative, protecting the plate from oxygen, or at farthest catalytic, aiding the reduction as sulphuric acid aids the electrolysis of water. Upon the state of association of the gases in what is generally called mixture I venture an opinion with the greatest diffidence. I have always inclined to the opinion that the difference between physical admixture, as it is termed, of gases and chemical union, is one of degree, and the views of Dalton ever presented to my mind grave difficulties.* My present results seem to me in favour of the chemical view, as otherwise we can scarcely imagine electricity as effecting in the instances given a merely physical separation; it may, indeed be said that there is composition and decomposition produced by the same discharge, but this is very difficult to conceive, and can hardly apply to the cases of oxygen with nitrogen and of carbonic oxide.

* *Phil. Trans.*, 1843, p. 112.

In the experiments I have detailed, the flame or visible effect of the electric discharge coincided with the chemical effect; when the plate was positive, a small globule of flame of a purple colour was visible on the part of the plate attacked, and a bluish flame extended over an inch or more of the needle. When the plate was negative, a wider and less defined disc of blue flame extended over the part of the plate opposed to the positive point, like a splash of liquid thrown upon it, and a pencil of light appeared on the point. Sometimes, but not always, this flame avoided the oxidated portion, probably from its inferior conducting power; and when this was the case reduction took place in a much slighter degree, or not at all; sometimes, and I observed this particularly with bismuth, the flame attached itself to the oxidated portion, and then reduction immediately followed. Here, as in all the electrical phenomena that I can call to mind, we get the visible effects of electricity associated with physical changes in the matter acting, changes of state in the terminals, polarisation of the intervening medium, or both.* These experiments furnish additional arguments for the view which I have long advocated, which regards electricity as force or motion, and not as matter or a specific fluid.†

The chemical polarity of gases shown, as I believe, in this paper, associates itself with an experiment which I made known in a lecture at the London Institution in the year 1843,‡ and which was subsequently verified by Mr. Gassiot§ with more perfect apparatus than I possessed, viz. that when discs of zinc and copper are closely approximated, but not brought in contact, and then suddenly separated, effects of electrical tension are exhibited, the one disc making the electroscope diverge with positive, and the other with negative electricity, showing that the effects ascribed by Volta to

* Gases at present believed to be elementary probably undergo a *quasi* chemical polarisation by electricity; thus portions of oxygen are changed to ozone, &c. See a recent paper by MM. Fremy and E. Becquerel, *Comptes Rendus*, Paris, March 15.—Note added to the proof, W. R. G., 1852.

† *Printed Lecture at the London Institution*, 1842, p. 28. *Correlation of Physical Forces*, p. 48.

‡ *Lit. Gaz.*, 1843, p. 39.

§ *Phil. Mag.*, October 1844.

contact can be produced without contact, and by mere approximation, the intermediate dielectric being polarised, or a radiation analogous, if not identical, with that which produces the images of Moser taking place from plate to plate.

The present experiments also associate themselves with the gas battery, where, though an electrolyte is used as the means of making the action continuous, or producing what is called current electricity, the initiating effect is gaseous polarity, the films of gas in contact with the respective plates of platinum having antithetic chemical and electrical states.

The results detailed in Experiment 13 appear to open a new field of research. Priestley observed concentric circles produced by the electrical discharge from a powerful Leyden battery, which he describes as consisting of minute cavities and globules of fused metal.* In my experiments there is an alternation of oxidation and reduction, a medium capable of producing both being present; the lateral effect and complementary colours have to my mind something closely resembling the phenomena of interference in light, although, from the polar character of the force, it is difficult to imagine any precisely analogous condition of electricity. The discharge taking place from different parts of the needle, and extending from its point to a considerable distance over its surface, would give different lengths for the lines of polarisation and discharge to the different parts of the disc on the silver plate affected by the discharge; and assuming electricity to be propagated by undulations, there would be interference; but instead of alternations of light and darkness we get alternations of positive and negative electricity. The ring of polished metal between the central spot and the exterior ring quite distinguishes these rings from the ordinary colours of thin plates, *i.e.* colours the annular succession of which depends only on the different thicknesses of the film; here doubtless the colours of the oxidated portions are colours of thin plates. Experiment 14 shows clearly that the action by which the polished ring is formed is a polar action of the discharge, and not a mere absence of action.

* *History of Electricity*, 2nd edition, p. 624.

When the plate is negative the effect is, as I have observed, less marked and more uncertain ; but in this case it should be recollected that the visible discharge issues from the point, and does not extend, or extends to a very small degree, over the surface of the needle.

If the phenomena were such that the central portion were always clear, while around it was one, and one only, circle of oxide, it might be accounted for by the hypothesis that the lines of polarisation and discharge between a point and flat surface assume the form of a hollow cone ; but a cone of negative bounded by cones of positive action still gives the idea of some lateral fits or phases of undulation.

The high rarefaction of the medium by the discharge, and its intermitting character, might occasion pulsations by the in-rushing of the surrounding gas, and thus vacua in circles might be formed at the places where the action of oxidation is rendered null ; but this view is, I think, inadmissible ; it does not account for the effects obtaining only in certain mixtures, it does not account for the reducing action, the plate being positive, and presents other difficulties. The point involved in Experiments 13 and 14 presents apparently a wide field of enquiry ; I therefore will not farther dilate on it at present, and hope to make it the subject of future investigation.

Postscript, April 24.—I may, I trust, be permitted to add to this paper one or two experiments on the subject last discussed. Assuming that the alternations of oxidation and reduction were produced by interference in consequence of the discharge proceeding from successive points of the terminal or terminals, a difference of effect might be anticipated if the electricity passed from a point only, and not from a line, as was the case in Experiment 13. I therefore sealed a platinum wire $\frac{1}{60}$th of an inch in diameter into a piece of glass tubing, and then ground the extremity to a flat surface, so that the section only of the wire was exposed ; this wire was placed opposite, and at 0·07 of an inch distance from the polished silver plate, in a mixture of one volume of oxygen with five volumes of hydrogen attenuated until the barometer stood at half an inch ; discharges from the secondary coil were then

passed, the plate being positive, and a round dark spot of oxide formed, represented at fig. 8; the platinum sealed in glass was then removed and the steel needle substituted for it, all else, viz. plate, gas, barometer height, &c., being the same. The system of rings represented at fig. 9 was now produced.

Another experiment was made, directed to the same point: a wire of copper 0·04 inch diameter, and a thread of glass of the same diameter, were attached by sealing-wax at their extremities in a horizontal position 0·025 of an inch from different parts of a silver plate, being insulated from the silver by the wax interposed at the extremities. The gaseous mixture and barometric height being the same as in the last experiment, and the silver plate made positive, when the platinum wire sealed in glass was brought near the plate, and the discharges passed, a spot similar to fig. 8 was formed; but when the coated point of platinum was brought over the copper wire at 0·02 inch distance, a figure consisting of two separated semicircles was formed, having spots in the bisection of the chords, as shown at fig. 10, the portion between the spots and the semicircular line of oxide being of polished silver. With the glass thread the effect was the same, but produced with greater difficulty and not so well defined.

In many repetitions of these experiments which I have made I have invariably produced the alternately polished and oxidated rings from the bare wire, and have not procured them from the coated wire, except to a very slight degree, and under certain circumstances, which, as far as I could trace, were as follows:—

1st. When the extremity of the wire was very near the plate, so that it had a sensible magnitude with reference to the intervening space, a slight formation of minute rings could be detected at the commencement of the experiment.

2nd. When the experiment was long-continued, or when the coated platinum wire had been used for previous experiments, a set of rings, not consisting of an alternation of oxidated and polished rings, but of annuli of different degrees of oxidation, were formed.

When the experiment is continued for some time a dark deposit is formed on the glass around the extremity of the

platinum wire, giving an extended conducting surface; and this may be the reason why such rings are formed, though these rings, in all the cases which I have observed, differ broadly from the rings formed by the bare needle or wire, not having the interposed spaces of perfectly bright silver; and in all the cases the difference of effect produced by the coated and the bare wire is very marked; in by far the greater number of experiments, when proper precautions are taken, not the slightest formation of rings takes place with the coated wire; with the bare wire, in the gaseous mixture last mentioned, I have always seen them formed.

Thus there are three systems of rings which may be formed by the discharge. First, rings such as those seen in the ordinary cases of thin plates; these I have only observed with olefiant gas, though probably there are many other conditions in which they may be produced. Secondly, rings formed by the superposition of layers of oxides, possibly arising from the fact that at certain definite periods portions of the plate become by oxidation inferior conductors, and other portions are attacked, and being at a different distance undergo a different molecular change by oxidation. Thirdly, and to me far the most interesting set of phenomena are presented by the rings alternately bright and oxidated, showing effects of oxidation and reduction by the same current on the same plate, and which only take place in certain gaseous mixtures, of which, up to this time, one volume oxygen + five volumes hydrogen is the most efficient which I have obtained.

I cannot at present see any better mode of explaining these phenomena than by regarding them as analogous to the phenomena of interference in light, though doubtless, if this be a right view, the very different modes of action of light and electricity would present very numerous phenomenal distinctions. Alternations of opposite polar electrical actions in the discharges passing in the same direction are, I think, very clearly shown in these experiments, and this appears to me a result worthy of attention.

Though acquainted with Nobili's beautiful experiments on the formation of coloured rings by deposition in electrolysed liquids, yet as I was working on gases it did not occur to me

to refer to his memoirs ;* I have done so since making the experiments given in this Postscript, and find that with regard to the rings so formed by electrolysis he suggests interference as a possible explanation.

The dark space in the discharge to which Faraday has called attention may possibly be connected with these phenomena. I have observed that in a well-exhausted receiver, containing a small piece of phosphorus, the discharge is throughout its course striated by transverse non-luminous bands, presenting a very beautiful effect, and a yellow deposit, which, as far as I have yet examined it, seems to be allotropic phosphorus, is deposited on the plate of the air-pump and on the neighbouring substances ; to show this effect well the needle should be positive and the plate negative, and the distance between them about an inch.

I could dilate much farther on these experiments, but have already trespassed perhaps too far for a postscript. Variations in the form of the terminals, in the nature of the gas, vapour, or gaseous mixture, in the density of the gas, in the intensity and quantity of the discharge, in the nature of the plate, &c., will occur to those who may feel inclined to pursue these experiments, and, if I am not over-sanguine, promise results of much interest.

ADDITIONAL NOTE ON THE DARK DISCHARGE.

Phil. Mag., Dec. 1852.

I find the transverse dark bands can be produced in other gases when very much attenuated, probably in all ; and I rather think the reason why they are more easily seen in the phosphorus vapour is, that all the oxygen having been consumed, a better vacuum is formed.

In addition to these bands, and under circumstances where they are barely visible, there is always seen a well-defined dark space intervening between the glow surrounding the negative, and the stream of light proceeding from the positive terminal ; it appears independently of the length of the discharge, though

* *Ann. de Ch. et de Phys.*, vol. xxxiv.

a space of an inch is a convenient distance for exhibiting the effect well.

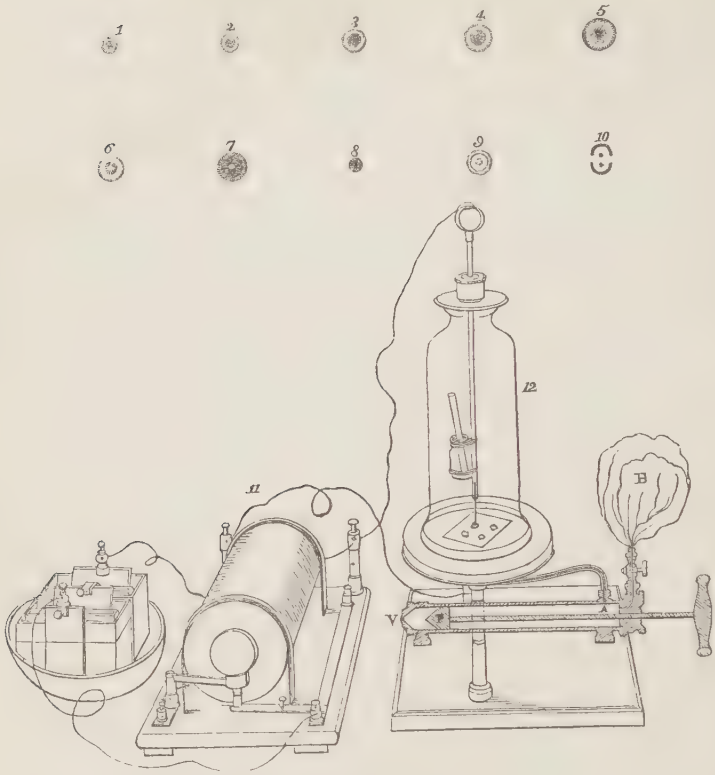
This dark discharge is elaborately described by Faraday as produced by the ordinary electrical machine, Experimental Researches, § 1,544 *et seq.*

Having in my mind the analogy of interference, it seemed to me that this dark space might be due to the crossing of the lines of discharge from the successive points of the needle, the knob, or plate from which the negative discharge issues.

As the positive discharge appears to issue from a point, and not to surround the wire, as does the negative, the position of the dark space in close approximation to the negative terminal was in favour of this view; if correct, it should follow that if the terminals were coated points instead of wires, knobs, or plates, this dark space would not be observed, or its position would be changed. Experiment verified this expectation: when platinum wires sealed in glass were employed and a good vacuum formed, the line of luminous discharge was continuous when the platinum points were brought to a distance of half an inch.

When these terminals are so far separated as not to give a continuous line of discharge, a pencil appears on each terminal, which gradually becomes fainter and fainter towards the middle of the intervening space; and if the distance be great, the discharge ceases to be luminous towards the middle of the intervening space, from excessive diffusion; but this will be seen to be a very different effect from the abrupt and well-defined dark space which appears in close approximation to the negative terminal when the coated wires are not employed.

When the positive terminal is coated and the negative one bare, the dark space appears on the point of the bare wire, the wire itself being surrounded by a lambent flame; but with the converse arrangement there is no such dark space. All this is much in favour of interference taking place, the coincidence of positive and negative phases of the discharge producing at certain points mutual neutralisation.



DESCRIPTION OF PLATE.

Figs. 1 to 10 show the spots and rings in the order referred to: it should be observed that printed figures give but a very imperfect notion of the actual effects.

Fig. 11 is the coil apparatus, the contact-breaker being in front.

Fig. 12. The air-pump, of a construction which I proposed many years ago, and have found most useful for electrical or chemical experiments on gases.

P. An imperforate piston, with a conical end, which, when pressed down, fits accurately the end of the tube, the apex touching the valve V, which opens outwards.

A. Aperture for the air to rush from the receiver when the piston has been drawn beyond it.

B. Bladder containing the gas to be experimented on.

The piston-rod works air-tight in a collar of leathers, and the operation of the pump will be easily understood without further description.

If it be required to examine the gas after experiment, a bladder or tube leading to a pneumatic trough can be attached at the extremity over the valve V.

ON THE STRIÆ SEEN IN THE ELECTRICAL DISCHARGE IN VACUO.

Phil. Mag., July 1858.

In a paper communicated to the Royal Society in January 1852 I published the first notice of transverse bands or striæ which are seen in the discharge from a Ruhmkorff coil in the vacuum of an air-pump, wherein is placed a piece of phosphorus; and in a postscript to that paper ('Phil. Mag.,' vol. iv.) I stated that the striæ could be seen in several attenuated gases, probably in all. Some time after the publication of my paper, but without having seen it, M. Ruhmkorff observed the same phenomenon in the vapour of alcohol; and it has subsequently been experimented on by Messrs. Masson, Quet, Du Moncel, Robinson, and Gassiot, the last of whom has elaborately examined it in the Torricellian vacuum.

No satisfactory *rationale* of the phenomenon has been given. In my first paper I gave no opinion as to its cause; but subsequently, in a communication to the British Association at Hull, I stated my belief that it was closely connected with the interruptions of the contact-breaker. The following is the notice of my communication in the 'Athenæum,' August 30, 1856: 'Mr. Grove has observed that the mode of breaking contact has a marked influence on the phenomenon. If, for instance, the arm of the contact-breaker be made to rest on a slight spring placed underneath it, the bands become narrower. If a single breach of contact be effected, most observers have remarked that the effect is still perceptible; but it is very difficult to effect a single breach of contact; the fusion of the metals at the point of contact, with the vibration accompanying the movement, occasions a double or triple disruption. The best mode is to place two stout copper wires across each other, and with a firm hand draw one over the other until the end of the former parts company with the latter; when this is well done the striæ are, in the majority of cases, not observed. Of all the substances which have been tried, the vapour of phos-

phorus succeeds best; and with this is seen a remarkable effect on the powder or smoke of allotropic phosphorus (which is always formed when the striæ are observed): this smoke traverses from pole to pole from the negative to the positive side, showing, unless there be some latent optical deception, a mechanical effect of the discharge under the circumstances.'

Dr. Robinson, who has a little misunderstood my meaning in this passage, says ('Proc. Royal Irish Acad.,' December 1856): 'Mr. Grove appears to think it arises from some vibration in the metal of a contact-breaker, which produces a fluctuation in the inducing current.' I did not mean to say that the effect was due to any peculiar vibration of the metal of the contact-breaker, but to the interrupted and successive discharges of the apparatus itself—that the changes in the character of the discharge attendant upon changes in the action of the contact-breaker, as well as the frequent absence of striæ when only a single disruption was effected, afforded evidence that the striæ were connected with the multiplied discharges. My notice was, however, short and somewhat obscure, as, although I entered fully on the matter in the Section-room, I had prepared no memoir on the subject. I still retain the opinion I then expressed, though I do not assert it as a positive conviction; the difficulty of proving it arose from the circumstance that it was next to impossible to produce, by the ordinary modes, a single discharge from the induced coil, for the following reasons. When one extremity of the wire of the primary coil is separated from the other, an induced current is produced at the moment of disruption in the secondary wire and a consequent discharge in the vacuum. But at this same moment of disruption, the extra or induced current in the primary wire itself finds a passage in the form of a spark immediately the contact is broken, and this extra current occasions necessarily a second induced current in the secondary wire, which, having a ready path opened to it by the discharge of the previous current, would be discharged through that path, though it might not have tension enough to overcome any great resistance. Although, therefore, these discharges cannot be separated to the eye, it by no means follows that

there are not two discharges when an apparently single disruption of the primary circuit takes place.

I had repeatedly and in vain attempted to get rid of this difficulty, and to produce what I could feel assured was a single induced discharge, when a very simple plan occurred to me, which I am surprised did not more readily present itself; it is to make an interruption in the secondary circuit, besides that formed by the vacuum, such second interruption to be in air, and of the extreme length of the spark, so that not more than one spark should pass at a time across the intervening air, and consequently not more than one discharge through the vacuous space.

Using the air-pump with phosphorus and a stick of potash in the receiver, I connected by a wire one of the secondary terminals of a Ruhmkorff coil with the rod which entered the receiver at the top. The wire from the second terminal was attached to a glass stand or pillar, so that its end projected; to another glass stand was attached a wire with one end also projecting, and the other end was fixed to the brass-work of the air-pump. The two projecting ends could be made to touch, or to remain at any desired distance from each other.

The ends of the wires being in contact, the coil apparatus was set at work, and the striæ very beautifully exhibited in the receiver; the projecting wires were now gradually separated, and the striæ for some time continued visible and until the points of the wires were so far apart that an occasional spark only passed from point to point; the striæ now disappeared, and a uniform luminous cloud was produced in the receiver. Even in this extreme case, however, there is apt to be a double discharge, which anyone who has not watched closely the phenomenon with eye and ear would not detect, but would pronounce the discharge to be single.

From unavoidable minute differences in the action of the contact-breaker, discharges will, all conditions being apparently the same, vary in strength and length; those accustomed to the apparatus will know that, by separating the terminals beyond the normal distance of the discharge, sparks will still pass at occasional, distant, and irregular intervals, proving the slightly varying intensity of the apparatus.

Now, when the points are separated to the maximum for the experiment which I have detailed above, two classes of sparks will be perceived at different times in the air—the one a thin blue spark, giving to the ear a single sharp sound, and the other a burred yellow spark, with a sound not so clear and metallic, but attended with a slight whizz: this contrast of sound may be roughly compared to that presented by a whole or cracked piece of porcelain when struck; it requires attention and habit to distinguish these two classes; but when the power is attained, it will be seen that with the first or single discharge there are no striæ in the receiver; with the second, which I believe to be a double discharge, striæ are visible.

By careful adjustment I have been able to obtain sparks of the former class only, and then the experiment may be continued indefinitely with no striæ visible; but on the slightest alteration in the distance the striæ appear. It is, however, more frequently the case that, notwithstanding all the care used, sparks of both characters will pass in irregular succession; and then the discharges will be without striæ in some cases, and with them in others.

It is necessary to mention the above distinctions, as otherwise any person repeating the experiments might be deceived, and not succeeding in obtaining discharges without striæ, though the discharges were apparently single, would imagine the results I have described to be illusions arising from an error of vision.

I repeated the above experiments in one of Mr. Gassiot's vacuum tubes, which showed the striæ beautifully under ordinary circumstances; but when the division in the circuit was carefully made and carried to its fullest extent, the discharges passed without any striæ, the tube being filled at each discharge with a uniform glow.

From the above experiments I am satisfied that in attenuated media wherein the striæ are well seen when the ordinary interrupted currents are used, discharges may be made to pass which exhibit no striæ, and under such circumstances that there is every reason to believe single discharges only pass. Join to this the fact that the appearances of the striæ vary with any variation of the contact-breaker, that the striæ are

more numerous and narrower in proportion as the intermissions of the contact-breaker are more rapid, which I have observed to be very notably the case, the evidence seems strong, if not conclusive, that the striæ or bands are due to the mechanical interference or reciprocal impulsions of two or more discharges, or rather of the impulsions on the medium affected.*

I will take this opportunity of mentioning one experiment which I believe to be new and not devoid of interest. M. Plücker and Mr. Gassiot have published several interesting experiments on the effects produced by magnets on the striæ or on the electric discharge in highly attenuated media, commonly called vacuua.

In repeating some of these experiments with a vacuum-tube kindly given me by Mr. Gassiot, two feet nine inches long between the platinum wires, and one inch internal diameter, I found that when the pole of a powerful bar magnet was brought close to the positive platinum in a certain direction, the visible discharges only passed occasionally and at intervals through the tube, notwithstanding the apparatus went on working, and the contact-breaker vibrating as usual. After many experiments, I found that I could stop the discharge entirely, by placing the poles of two magnets opposite each other and in a particular direction with reference to the line of discharge. The two platinum wires in this tube are inserted perpendicularly to the axis, and penetrate half an inch into the tube. If, now, the tube be placed horizontally, the platinum wires uppermost, and the observer looking along the tube from the positive to the negative wire, the north pole of the magnet should be placed on the right-hand side, and as nearly as can be to the positive platinum, *i.e.* touching, or nearly so, the glass tube, while the south pole of another magnet is similarly placed on the opposite or left-handed side. The discharges cease entirely, and there is no conduction through the tube, showing that it is not a case of dark discharge, but an arresting of the passage of the discharge through the

* This conclusion is too limited. Mr. Gassiot subsequently to the publication of this paper obtained the striæ with the voltaic arc. This is, however, regarded by some as intermittent, and is at all events not a single discharge.—W.R.G., 1874.

attenuated medium. If the tube be turned one-fourth round its axis in either direction, the discharges reappear, as they also do if the magnets be reversed. The experiment requires care, and it seems to be necessary to make it with a power of coil just sufficient to enable the discharge to traverse the distance between the platinum terminals and with a certain magnetic power : if made with a powerful electro-magnet, the discharge is stopped, whichever side the poles be placed with reference to the platinum wires, but the discharge is only made intermittent when the platinum wires are placed in a line joining the poles of the magnet. In the above-mentioned tube the portion of one end of the glass around one of the platinum wires was covered with a dark deposit of platinum, from the terminal at this end having been constantly used as the negative pole ; when this was made positive, instead of the clean end, the effect of arresting the current could not be produced : this circumstance, and the delicacy of the conditions necessary to ensure success in this experiment, lead me to think that the stoppage of the discharge is due to the diversion in direction of the electric line of force produced by the magnet; the discharge is, so to speak, blown out by the magnet ; but I hesitate to express a positive opinion on this point.

When magnets are made to approach the negative platinum there is no stoppage of the discharge, but attractions and repulsions are observed on the blue flame, which have been described by Mr. Gassiot, and which I need not therefore repeat.

I tried the effect of magnets on the spark from Ruhmkorff's coil in air, but could not arrest them, though they appeared to pass with somewhat greater difficulty and longer intermissions ; the difference was, however, very slight.

ON SOME ANOMALOUS CASES OF ELECTRICAL DECOMPOSITION.

Philosophical Magazine, March 1853.

WOLLASTON showed, in a well-known experiment,* that when Franklinic electricity from the prime conductor of an ordi-

* *Phil. Trans.* 1801, p. 431.

nary electrical machine was made to pass from fine points of gold wire sealed into glass tubes and immersed in water, both the positive and negative pole yielded mixed oxyhydrogen gas, the exact proportions of which are not given. This experiment has been subsequently discussed by Ritchie,* Faraday,† and others; but differing, as the decomposition in this case does from electrolysis by the voltaic battery, I am not aware that it has been repeated on a large scale, or that anything more than a mere verification of the fact has been attempted.

The apparatus constructed by M. Ruhmkorff, which I have described in a paper in the 'Philosophical Magazine' for December last, p. 500, having given me the means of procuring electricity of tension in quantity far exceeding that of the best electrical machine, and having my attention directed to Wollaston's experiment by the wires sealed in glass tubes which I used for my recent experiments, I determined to make some experiments similar to Wollaston's, but with the spark from the secondary coil instead of that from the electrical machine.

In the experiments which I am about to detail the terminals of the secondary coil consisted of two wires of platinum, $\frac{1}{60}$th of an inch in diameter, sealed into glass tubes. One of these coated wires was prepared on purpose for these experiments; and, having been carefully sealed into a glass tube, the extremity was ground on a hone until the section of wire formed one surface with the glass.

The other coated wire had been similarly prepared, but had been used for some time for the experiments in attenuated gases, given in the paper to which I have alluded; and the extremity of the wire was worn beneath the surface of the glass, a circumstance which proved of some little importance.‡

The tubes containing the platinum wires were curved in form, something resembling the letter Z (see fig. 1), the extremities immersed in a porcelain capsule containing the liquid

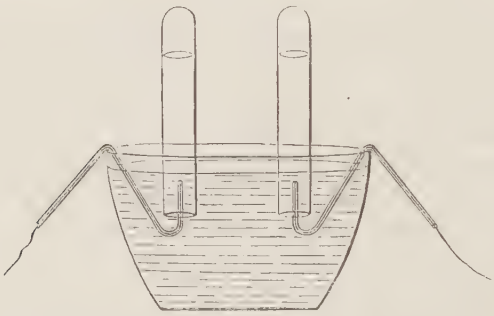
* *Phil. Trans.* 1832, p. 282. † *Ibid.* 1833, p. 23.

‡ This result is important in another point of view than that relating to the experiments in this paper, viz. it shows that even in the aurora borealis experiment, or passage of the spark in highly rarefied gas, solid matter, even such as the dense substance platinum, is given off; thus favouring the theory that the spark is ignited matter, and rendering the hypothesis of a fluid unnecessary for its explanation.

to be experimented on, a graduated test-tube filled with the liquid being inverted on the extremity of each.

From the Z form of the terminals the extremities projected $\frac{3}{4}$ths of an inch into the test-tubes, so that no gas could possibly escape. At first I tried distilled water, which had been boiled immediately before the experiment to deprive it of air; I found, however, that from its want of conducting power, only an infinitesimal decomposition was produced; a few drops of sulphuric acid were added, and immediately a notable evolution of gas took place, a spark being visible at the end of the wire which was worn beneath the surface of

Fig. 1.



the glass, whether this wire were positive or negative, and none on that which was even with the glass. This spark had a material influence on the relative quantity of gas given off by each terminal, the gas being generally less from the spark-giving terminal than from that which did not yield a spark; and when both were constructed so as to give sparks, the quantities of gas were irregular, and bore no definite relation to each other. Thus, in one experiment the gas from the negative was to that from the positive as 2·3 : 1; in another experiment the gas from the positive was to that from the negative as 2·5 : 1; frequently the quantities from each were nearly equal. It is obvious that when the wire was depressed below the surface of the glass a film of gas must intervene between the wire and the liquid, across which film the spark passes. When the wire is even with the glass the gas escapes more readily, and there are fewer or no sparks.

There is no doubt that as the bubble of gas forms on the point of platinum the spark recombines a portion of it, and the number of sparks, the size of the bubble preceding the spark, and the quantity recombined, would vary indefinitely in each different experiment, and even in the course of the same experiment.

I now give a selection of experiments from my note-book, with the volumes and analyses of the gases evolved. The decomposition was arrested in each case when the larger volume of gas had reached to 0·3 c. i. The analyses were made by an eudiometer, which I have formerly described,* consisting of a loop of fine platinum wire sealed into a graduated glass tube, and heated or ignited by the voltaic battery:—

1st. The gas from the positive pole contracted to 0·4 of its original volume; the residue, mixed with half its volume of oxygen, detonated, leaving a minute residue, apparently nitrogen. The gas from negative pole, 2·3 times the volume of the positive, contracted to 0·15 of its volume; the residue contracted very slightly on the addition of hydrogen.

2nd. Gas from positive, 2·5 volumes contracted to 0·7 of its volume.

From negative, 1 volume contracted to 0·4; the residues mixed contracted again to 0·7; and this residue, mixed with half its volume of oxygen, detonated, leaving only a very minute bubble.

3rd. Instead of two coated wires one coated wire was employed, and the opposite terminal consisted of a strip of platinum foil, $\frac{1}{4}$ inch width and 4 inches length, exposing consequently, reckoning both sides, 2 square inches of surface.

This strip was attached to a platinum wire, which was sealed into a glass tube, the extremity of the platinum foil being $\frac{1}{2}$ an inch within the orifice of the tube, so that no gas could escape. (See fig. 2.)

With the coated point positive, the platinum plate negative, 0·3 c. i. of gas was collected from the point; a minute bubble only appeared in the tube containing the strip or plate;

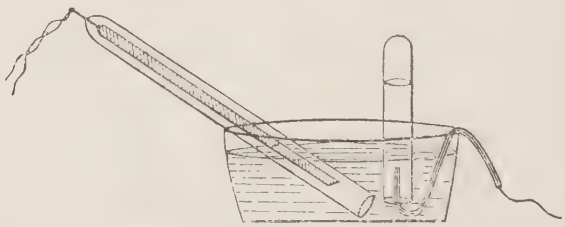
* *Phil. Mag.* Aug. 1841. *Phil. Trans.* 1847, p. 1.

the gas contracted in the eudiometer to 0·7 of its volume; the residue, mixed with half its volume of oxygen, detonated, leaving a very small residuum.

4th. In the converse experiment, *i.e.* coated wire negative, plate positive, 0·3 c. i. of gas was collected from the negative coated wire, and, as in the former case, only a minute bubble appeared in the tube containing the plate, and far too minute for analysis; the gas from the negative point contracted to half its volume in the eudiometer; the residue was hydrogen, as before.

5th. In the above experiments I had used from 2 to 3 oz. of distilled water, with a few drops of sulphuric acid added to

Fig. 2.



improve its conducting power, without paying any particular attention to the exact quantity of acid which was mixed.

I now determined to try different degrees of dilution of the acid, and to note more accurately its proportion to the water. I first took the extreme case of dilution, using 3 oz. of distilled water, and merely touching it by a narrow glass rod which had been dipped into sulphuric acid.

The coated wire was the positive pole, the plate of platinum foil the negative; 0·3 c. i. was collected from the coated wire, and not a trace of gas was perceptible in the tube containing the plate. The gas contracted to one-half its volume in the eudiometer, and the residue, mixed with one-half its volume of oxygen, contracted to 0·3 of its original volume; the residue of this second contraction appeared to be nitrogen, and its greater quantity in this than in the previous experiment was doubtless due to the greater time which, from the inferior conducting power of the solution, was necessary for the decomposition, more air having thus entered into solution.

In this experiment the liquid was exposed for twenty-four hours, during ten of which the discharges were kept up, while in the previous experiments less than an hour was sufficient, and the water had not cooled from its preliminary boiling before the experiment was completed.

6th. I next tried a strong solution of sulphuric acid, specific gravity 1·23; the gas from the coated point, which was positive, contracted to 0·2 of its volume; the residue was hydrogen.

7th. I used a still stronger solution, specific gravity 1·45, both points being coated; the volume from the negative pole was to that from the positive as 4·5 to 3·25; the gas from the negative pole contracted in the eudiometer to only 0·95 of its volume; the residue was pure *oxygen*; the gas from the positive pole contracted to 0·5; the residue was hydrogen. Here, to my surprise, there was, after the first contraction, a residuum of oxygen in the ratio of more than 2 volumes to 1 of the residuum of hydrogen. The experiment was repeated with acid of specific gravity 1·5; from the positive point 3·3 volumes were collected, from the negative 2·6; the gas from the negative pole contracted to 0·9 of its volume; the residuum was pure oxygen; that from the positive contracted to 0·4 of its volume, the residue being hydrogen.

8th. I now tried a saturated solution of sulphate of copper with a few drops of sulphuric acid added; a remarkable difference was shown in the conducting power of this solution as compared with the dilute acid; the contact-breaker of the primary coil gave very minute sparks, unattended with the usual snapping noise; it was much as if the terminals of the secondary coil had been united by a metallic connection; and the superiority of the conducting power of the solution of sulphate of copper in this experiment over dilute sulphuric acid was far greater than is shown in ordinary electrolysis by the voltaic battery. The coated point was positive, the plate negative; the gas from the positive point detonated, leaving a very slight residue; but on the interior of the collecting tube, near the point, was a deposit of pulverulent metallic copper. On the negative plate there was no deposit, and no gas was given off from it. Here the copper deposit was evidently a reduction occasioned by the excess of hydrogen, but there

was no symptom of any effect produced by the missing equivalent of oxygen.

9th. With a solution of caustic potash the coated point, which was positive, gave a gas which contracted to 0·32 of its volume, the residue being hydrogen; the plate, which was negative, gave off a mere trace of gas.

10th. Two plates were used as the terminals of the secondary coil in dilute sulphuric acid, instead of a plate and a point. No gas was given off from either, though the experiment was continued for several hours.

11th. I could detect no difference in the solutions employed before and after the experiment; but as the sulphuric acid might be expected to mask the effect of any oxygenated compound which might result from the absorption of oxygen, which took place in all these experiments, except those where the strong solutions of sulphuric acid were employed, and as pure distilled water would give no effect, I tried the effect of the spark passed through aqueous vapour. Some distilled water was boiled and placed under the receiver of an air-pump, with a coated point and plate of platinum enclosed in a tube filled with the liquid, and inverted; the point and plate communicating respectively with the upper and under plate of the receiver, and thence brought into metallic communication with the ends of the secondary coil.

The receiver was then exhausted, and the vapour which had formed at the top of the tube soon dilated sufficiently to expose the point and plate; the spark was then passed across the vapour, and a permanent increase in volume of the vapour was soon detected. The experiment was continued for a week, the apparatus working five hours each day; at the end of that time much of the gas formed had bubbled out into the receiver, and on letting in the air the water rose in the tube until a bubble of gas of 0·03 c. i. remained in the top; this was analysed, and proved to be pure *hydrogen*.

On the interior of the tube near the platinum point was a dark pulverulent deposit, far too minute in quantity for analysis, but which had evidently proceeded from the platinum.

The only possible mode in which I can account for this experiment is, that this deposit consisted of an oxide or per-

oxide of platinum, and to account for it thus it must be assumed that platinum, like zinc or iron, will decompose water by abstracting its oxygen.

12th. I endeavoured, by working for a long time upon a limited quantity of water very slightly acidulated with sulphuric acid, to detect some compound which might be formed by the oxygen which disappeared.

I placed in a small tube 0·15 c. i. of distilled water, touched with a rod which had been smeared with sulphuric acid. I continued working on this for a fortnight, averaging five hours a day; and as the liquid diminished by evaporation I added distilled water which had been recently boiled. No change could be detected in the liquid at the expiration of this period. It gave an acid reaction with test-paper, precipitated chloride of barium, and showed no bleaching properties, which I looked for as a result of absorbed oxygen.

I have applied the expressions positive and negative to the terminals of the secondary coil which bore this relation to each other upon breaking contact of the primary coil: as far as I could ascertain by direct experiment, there was, on making contact, no spark or decomposition from the secondary coil; but although from the time which the magnetic coil takes to acquire its magnetism no spark is visible with this apparatus on making contact, possibly some slight decomposition might then take place; this, however, though it is well to notice the point, is immaterial to the consideration of the results detailed in this paper, as, if the currents were in alternate directions, the proportions of the gases would be equally exceptional and anomalous.

I have made many variations of the above experiments, but it would be a needless waste of space to detail them. The following are the general results:—

1st. With distilled water, containing a small quantity of sulphuric acid, there is always a notable excess of *hydrogen*; in one case the oxygen was only one-fifteenth of the whole volume of gas evolved. With solution of potash a similar result takes place.

2nd. When the quantity of acid is increased to the point at which the electrolytic power of the solution is the best, the

excess of hydrogen is less; and probably if the exact point of the solution were attained, there would be no excess.

3rd. When the solution is much stronger than that which makes the best electrolyte, there is a notable excess of *oxygen*.

4th. When the electrodes exceed a certain size, water conducts without any apparent signs of decomposition.

On making a search for any previous experiments on this subject, I find that Faraday ('Phil. Trans.' 1834, p. 91) has observed that when a strong solution of sulphuric acid, formed by mixing two measures of oil of vitriol with one of water, was subjected to *ordinary* electrolysis, a remarkable disappearance of *oxygen* took place. The strength of the acid he then employed would be very nearly that employed by me in experiment 7; but in my experiment, when the solution is subjected to decomposition by the spark from the secondary coil, exactly the converse effect takes place, and there is a notable disappearance of *hydrogen*.

Faraday attributes the disappearance of the oxygen to the formation of peroxide of hydrogen; but in those of my experiments where oxygen disappeared, this could hardly be the case, as the gases are eliminated at a point of ignition at which this compound would be decomposed, and in many of my experiments the liquid was kept very nearly at the boiling-point.

I abstain at present from expressing any opinion as to the cause of these singular anomalies, and hope to make a farther communication on the subject, when I can resume and complete some experiments which I have in view, but which it will be some time before I can undertake.

THE ELECTRICITY OF THE BLOWPIPE FLAME.

Phil. Mag., January 1854.

VOLTA and Erman made known the first indications of the production of electricity by flame. Pouillet and Becquerel have experimented and reasoned on the statical effects of such electricity, while Andrews, and more recently Hankel and

Buff, have published very interesting results on the effects of flame as to conduction and production of voltaic electricity.

The experiments of which I am about to give a notice were for the greater part made before I had read the papers of the two last authors; and while they differ in results from theirs, they give a means of producing a voltaic current from flame far more distinct and powerful than any which I have tried or read of.

The flame I have worked with is that of naphtha or spirit of wine, urged by an ordinary glass-worker's blowpipe; and with a galvanometer, the needles of which are barely deflected to 2° by any current which I can procure by the flame of a common spirit-lamp, I can by the blowpipe flame procure deflections of 20° or even 30°, and with great certainty and uniformity of direction.

I am not aware that the blowpipe flame has ever been used for the production of electricity, though I see by M. E. Becquerel's recent paper that M. Becquerel, sen., has used it as a means of adventitiously heating a spiral placed in another flame.

Two platinum wires of six inches long and $\frac{1}{70}$th of an inch diameter have their ends formed into coils of $\frac{1}{8}$th of an inch long and wide; these wires are attached to copper wires insulated by glass stands, and having their farther extremities connected with a galvanometer. My galvanometer is by Ruhmkorff; the wire is not as long as those now constructed, being only 544 feet, but I have so magnetised the needles as to render them highly astatic; they take four minutes to make one oscillation.

An additional binding screw is connected with the wire at 54 feet of its length, so that I can ascertain by the same instrument the effect of a slighter resistance.

When the flame of a spirit-lamp is urged by the blowpipe one of the above-mentioned coils is placed in the full yellow flame just beyond the apex of the blue cone, and the other near the orifice of the brass jet, or at what may be called the root of the flame, just beyond the base of the blue cone, the distance between the two coils being $2\frac{1}{2}$ inches. The coil in the full flame is at a white heat and brilliantly incandescent, the

coil near the orifice or at the commencement of the flame is cherry-red. The galvanometer is deflected to an average of 6°, the coil near the orifice or at the root of the flame being positive, or related to the farther coil as zinc to platinum in the voltaic trough. On reversing the position of the wires the galvanometer is deflected 6° in the opposite direction.

This current is not due to thermo-electricity excited in the wires at the point of junction of the platinum and copper, for it is unaltered in direction by a powerful thermo-current excited in these by heating the points of junction by another spirit-lamp.

By making this thermo-current aid or counteract the flame-current a slight difference in degree is perceptible in the deflection, according as the point of junction of the one or other wire is heated, but no difference in direction.

The flame-current, moreover, scarcely affects the short-wired galvanometer, while the thermo-current of copper and platinum whirls the needle to 90°.

It is not a thermo-electric current arising from the unequal heating of the two coils, for it is in the same direction when the farther coil is removed from the full flame, so as to be less heated than the coil at the root of the flame. It is also different in direction from the thermo-current produced by unequally heating the coils in similar parts of the flame, or the current described by M. Buff, and to which he ascribes the electricity of flame. The flame-current proper in my experiments is opposed to and conquers the thermo-current.

On advancing the coil from the root of the flame towards the farther coil the deflection lessens, but still preserves its direction until the wires get very closely approximated, when the deflection of the flame-current yields to that of the thermo-current, and the direction of the needle depends on the relative heat of the coils.

A wire of zinc near the root of the flame with the platinum coil in the full flame gave a much smaller deflection, only 2°; when the direction of these wires was reversed the current was stronger, the galvanometer marking 5°; the direction of the deviation was in both instances the same as with platinum and platinum.

Iron and copper acted as zinc, but rather more feebly. As in these experiments the wires of zinc, iron, and copper respectively were stouter than the platinum wire, I attribute the superior amount of deflection when the oxidable metal was in full flame to the greater cooling effect of the thicker wire reducing the antagonising thermic current.

As the above experiments seemed to show that there was a proper flame-current irrespective of, and even overcoming the thermic flame-current, I was led to expect that by uniting in direction these two currents I might get more marked results. The following experiment, it will be seen, realised this expectation.

I formed a little cone of platinum foil of $\frac{6}{10}$ths of an inch in depth, and the same width at the widest part; I suspended this in a ring of platinum wire and substituted it for one of the coils.

Being placed in the full flame, the coil being at the root, it was filled with water, and water dropped into it from a pipette to supply that which was boiled away. I now readily obtained a deflection of 20° in the same direction as in my original experiment, and frequently the needles deviated to 30°. When the cone filled with water was placed at the root and the coil in the full flame the deviation was only 5°.

In all the above experiments with the blowpipe it will be seen that the direction of the current was, as far as a comparison can be instituted between the blowpipe flame and ordinary flame, the reverse of that indicated in the experiments of Hankel, excepting his experiment with the flame of hydrogen; and also the reverse of the greater part of the experiments of Buff, which he rightly attributes to thermo-electricity.

One result of M. Buff ('Archives d'Electricité,' vol. xvii. p. 275), when he places one wire in the centre and the other at the outer margin of the flame, is probably dependent on a similar cause to mine; though when he places the second wire in the full flame the current is in a contrary direction to that which I obtain.

My experiments prove, I think, distinctly that there is a voltaic current, and that of no mean intensity, due to flame, and not dependent on thermo-electricity.

I know of no better theory to account for these results than that which Pouillet applied to the effects on the condenser, viz. that it is the result of combustion, the platinum at the commencement of chemical action, or where the elements are entering into combination, being as the zinc of the voltaic battery; and that at the termination of combustion, or at the points where the chemical action is completed, being as the platinum of the voltaic combination.

Although there is a distinct thermo-current produced by the contact of two unequally heated bodies with flame, yet when we see, as in the above experiments, a marked current, in a contrary direction to, and overcoming that which is excited by the thermo-current in the flame, and also that at the points of junction of the wires away from the flame, I see no means of viewing the resulting current as a thermo current. The blowpipe flame, from its definiteness of direction, brings out most distinctly this current; in other flames, from the more confused circulation of the heated and burning particles, the results are less significant; and the various flame-currents counteracting each other, the thermic current obtains a predominance.

The current from the blowpipe flame, when the platinum in the full flame is cooled, is so marked, that I have little doubt, by attaching to a powerful pair of bellows a tube from which a row of jets proceeds, and alternating pairs of platinum in flames urged by the jets, a flame-battery might be constructed which would produce chemical decomposition and all the usual effects of the voltaic pile.*

ON A METHOD OF INCREASING CERTAIN EFFECTS OF INDUCED ELECTRICITY.

Phil. Mag., Jan. 1855.

In the course of last year I observed that, by connecting the coatings of a Leyden phial with the extremities of the secon-

* This was subsequently done, and the effects of the flame battery shown at an evening meeting of the Royal Institution.—See *Proceedings of the Royal Institution*, Feb. 3, 1854.

dary coil of Ruhmkorff's apparatus, a great increase in the brilliancy of the discharge could be obtained. Circumstances diverted my attention from it at the time, and I did not publish the experiment, though I believe Mr. Gassiot mentioned it in one of his papers. I have since heard that M. Sinsteden, in France, had made the same observation, though I do not know when, nor whether he has published his experiments.

The point which I now think may be worth insertion in the 'Phil. Mag.' is the conversion, by means of a Ruhmkorff coil, of an indefinite amount of voltaic into static electricity. If a small Leyden phial have its coatings connected respectively with the extremities of the secondary wire of a Ruhmkorff coil (the primary being, as usual, connected with the condenser of M. Fizeau, and two wires being attached to the terminals and brought within striking distance), the noise and brilliancy of the discharges are greatly increased, with generally a slight diminution in their length. If now the voltaic series be increased, the coil and Leyden phial remaining the same, but little increase in the length or brilliancy of the sparks will ensue, that is, provided the battery was in the first instance sufficiently powerful to give the maximum effect of the coil without the phial. For instance, if with a Ruhmkorff coil of the size now usually made, ten inches long by four diameter, four cells of two inches by four of the nitric acid battery be used, and a pint Leyden phial, but little increase of effect will be obtained by using eight or more cells, and the platinum at the contact-breaker would be rapidly destroyed by the sparks.

But substitute for the pint Leyden phial one of double the capacity, and it will be found that though this second phial was inferior to the first with a battery of four cells (giving shorter sparks, and fewer in a given time, though somewhat denser), yet it is far superior to the first with the battery of eight cells, and the sparks at the contact-breaker are no longer injurious.

By adding more coated surface, for instance, another phial, four more cells may be added, and increased effects will be obtained, and thus with *the same coil* the brilliancy of the discharge may be increased to an extent to which I have not yet found a limit. I obtained this result some months back; but

not having a large battery, I did not go beyond ten cells, which I found would well bear a jar of one square foot coated surface.

By the aid of Mr. Gassiot's more powerful apparatus, I have, with him, used thirty cells of the nitric acid battery, two inches by four, and five square feet of coated surface; the effects were very striking—a roar of voluminous discharge of 0·6 of an inch long increased to 1·5 inch when the flame of a spirit-lamp was placed between the terminals. I have never witnessed such a torrent of electrical discharges; the noise could not be borne long without great discomfort.

With the same voltaic battery, and an additional square foot of coated surface, the effect was somewhat diminished. Mr. Gassiot had not more than thirty cells available at the time of our experiments, so that I have not yet ascertained the limit to which this increase of power can be carried. I presume, however, there is a limit, for reasons which will be presently apparent.

The following precautions are essential to the success of the experiment:—

1st. The wire proceeding from the outer extremity of the secondary coil must be connected with the inside or insulated coating of the Leyden battery, if the battery is not wholly insulated. The reason of this is, that the outer extremity of the coil is the better insulated portion, and also that to which electricity of tension flies off; a good spark can, under ordinary circumstances, be obtained from the outer, but scarcely any from the inner terminal of the coil.

2nd. The distance between the hammer of the contact-breaker and the soft iron core should be made as great as practicable, at least one-eighth of an inch; this is an important point as to the theory and experimental results of the Ruhmkorff coil. Time, as is well known, is necessary for the development of electro-magnetism; and M. Matteucci, in his recent valuable book on induced electricity, has shown some remarkable results flowing from this fact. If the hammer be too near the core, the former is raised before the latter has time to be fully magnetised; and when a Leyden condenser is used, farther time is required for this to be charged. This

demand of time indicates the probable limit to the increase of power to which I have above alluded.

It is very curious to see the *absorption*, so to speak, of voltaic power by the Leyden battery; when the maximum effect for a given Leyden jar has been passed, the contact-breaker shows by its sparks the unabsorbed induced electricity which now appears in the primary wire; an additional jar acts as a safety-valve to the contact-breaker, and utilises the power, and so on.

It is a question of some interest why a jar charged in the ordinary way by temporary contact of the terminals of a secondary coil will only receive a very slight charge, and give a discharge of scarcely measurable length, yet, when permanently connected with the terminals, will give a long and powerful discharge. The following is the best theory I can offer:—At the moment of the inductive action or wave of electricity the same wire which is affected by the electric impulse is unable to conduct it back again, and thus to discharge the jar; while, when the jar is attempted to be charged in the ordinary way, the contact, however apparently of short duration, lasts longer than the single impulse of electricity, and so the coil in great part discharges the jar. Some such state of the wire as that I have suggested must exist at the moment of an induced current, as otherwise the wire would discharge itself, or, in other words, would never receive a charge or state of opposite electricity of great tension at its extremities. At one time I considered the explanation to be, that at the moment of breaking contact a portion of the induced electricity flies off across the discharging interval in the form of a spark, and thus enables the jar to discharge itself, just as the voltaic arc will pass across the path of an electric spark, though it will not pass through a measurable distance of interposed air without the spark. This theory, however, does not satisfactorily explain the great increase in the charge of the Leyden phial in the above experiments, as compared with the charge by contact.

3rd. It must be borne in mind that each coating of the Leyden phial must be connected with each terminal; the jar is not, as many have tried the experiment, to be interposed in the secondary circuit.

The number of discharges in a given time will depend upon the intensity of the battery, and its relation to the amount of coated surface ; the eye cannot estimate this, but a rough measurement of the rapidity of succession may be made in the following way :—Move across the line of discharge with a steady hand a strip of writing-paper : it will be punctured with a row of holes, which will be the more closely approximated in proportion as the succession of discharges is more rapid. By a disc of paper attached to an axis moving with a given rate of revolution this measurement may be made very accurate and useful.

Those who possess the coil apparatus will find it very convenient to have a plate of glass, coated on each side with tinfoil, placed in the base of the machine, and having strips leading from each coating to binding-screws, with which the terminals of the secondary coil can be connected at will.

EXPERIMENTS ON THE APPARENT CONVERSION OF ELECTRICITY INTO MECHANICAL FORCE.

Phil. Mag., January 1856.

In a communication made to the members of the Royal Institution, on Friday, January 25, entitled 'Inferences from the Negation of Perpetual Motion,' I showed an experiment which, with one or two others now added, may be thought worth recording in the 'Philosophical Magazine.' They are hardly, I think, likely to be deduced from our received electrical theories, though possibly not inconsistent with some of them.

My object was to show that when electricity performs any mechanical work which does not return to its source, electrical power is lost. The first experiment was made in the following manner :—A Leyden jar of one square foot coated surface has its interior connected with a Cuthbertson's electrometer, between which and the outer coating of the jar are a pair of discharging balls fixed at a certain distance (about $\frac{1}{2}$ an inch apart). Between the Leyden jar and the prime conductor is

inserted a small unit-jar of 9 square inches surface, the knobs of which are 0·2 inch apart.

The balance of the electrometer is now fixed by a stiff wire inserted between the attracting knobs, and the Leyden jar charged by discharges from the unit-jar. After a certain number of these (twenty-two in the experiment performed in the theatre of the Institution), the discharge of the large jar takes place across the $\frac{1}{2}$-inch interval; this may be viewed as the expression of electrical power received from the unit-jar. The experiment is now repeated, the wire between the balls having been removed, and therefore the 'tip' or the raising of the weight is performed by the electrical repulsion and attraction of the two pairs of balls; at twenty-two discharges of the unit-jar the balance is subverted, and one knob drops upon the other, but *no discharge takes place*, showing that some electricity has been lost, or converted into the mechanical power which raises the balance. By another mode of expression the electricity may be supposed to be masked or analogous to latent heat, and would be restored if the ball were brought back, without discharge, by extraneous force.

The experiment I believe to be new, and to be suggestive of others of a similar character, which may be indefinitely varied. Thus, two balls made to diverge by electricity should not give to an electrometer the same amount of electricity as if they were, whilst electrified, kept forcibly together. An experiment of this sort I have made since my lecture in the following manner:—To a thick brass wire, two feet long, insulated and terminated by knobs, are suspended, by fine platina wires, two pairs of discs of paper coated with tinfoil, and four inches in diameter. The apparatus is electrised in a dry atmosphere by sparks from a machine, and the discs of each pair respectively diverge. To one of the pairs a silk thread is attached, by which the discs can be forcibly approximated, and as often as this is done the divergence of the other pair increases.

Another mode of showing the same effect is the following: On the top of an ordinary gold-leaf electroscope place two brass plates, such as those commonly used for a condenser; connect them by a long fine wire, and electrify them by a rubbed rod of glass or sealing-wax, so that the gold-leaves

diverge. Now raise the upper plate by a glass handle: the leaves collapse in proportion as it is raised and again diverge as it is depressed. It should be recollected that the plates are electrified by the same electricity, and are always metallically connected by the fine wire, in which respect this differs from ordinary induction experiments. It may be said that here the mechanical force is given by the hand; but this is only in part: the repellent effect of electricity does part of the work, and would be therefore expended; it is analogically as though a man were to add his force to the piston-rod of a steam-engine, which would not prevent the loss of heat by the dilating steam. I had hoped to have carried the experiments farther and examined the relative quantities, but unfortunately I have no time for such enquiry, and must leave it to others who have more leisure.

NEW METHODS OF PRODUCING AND FIXING ELECTRICAL FIGURES.

Phil. Mag., January 1857.

A CLASS of figures produced on polished surfaces by electrising a metallic bas-relief, such as a coin or medal placed on glass, mica, or polished metal, was made known by M. Karsten,* who refers in his memoir to the previous results of Moser and Riess, the latter having given the name of roric figures to those produced by electrical discharges, on account of their becoming visible, as Karsten's did, when breathed on. M. Karsten states that he had but imperfectly succeeded in fixing these figures by exposing them to the vapours of iodine or mercury,† and that when an insulating substance was interposed between the object and the recipient plate the figures were not formed.‡

This class of experiments possesses much interest, as showing the molecular changes accompanying electrical phe-

* *Archives de l'Electricité*, vol. ii. p. 647; iii. p. 310; iv. p. 457.
† *Ibid.*, vol. ii. p. 651.
‡ *Ibid.*, vol. iv. p. 464.

nomena; and believing, as I have for many years, that electricity is nothing else but motion or change in matter, or a force and not a fluid, I have recently made some experiments to ascertain whether similar effects took place in cases where electrical light is visible upon insulated surfaces only, a great number of experiments having already shown that the particles of metals or conducting bodies are projected when the electrical spark proceeds from them.

M. Du Moncel has shown that when two plates of glass, coated respectively on their exteriors with metallic plates, are kept separate and electrised, a brilliant electrical light is seen between the plates.* I thought I might render evident the molecular change which I believed to be taking place on the opposed surfaces of glass in such cases, and the following experiments, selected from many others, will, I think, prove that this is the fact:—

1. Two plates of window-glass, 3 by $3\frac{1}{2}$ inches, were immersed in nitric acid, then washed, and dried by a clean silk-handkerchief until their surfaces gave a uniform flush when breathed on. Between these plates was then placed a piece of hand-bill printed on one side only; pieces of tinfoil rather smaller than the glasses were placed on the outside of each, and these coatings were connected with the secondary terminals of a Ruhmkorff coil. After a few minutes' electrisation the coatings were carefully removed, and the interior surface of the glass, when breathed on, showed with great beauty the printed words which had been opposite it, these appearing as though etched on the glass, or having a frosted appearance; even the fibres of the paper were beautifully brought out by the breath, but nothing beyond the margin of the tinfoil.

2. It now occurred to me that I might render these impressions permanent by the use of hydrofluoric acid, and a similar experiment was made, the naked plate of glass after electrisation being exposed over a leaden dish containing powdered fluor spar and sulphuric acid, and slightly warmed; the letters came out rather imperfectly, but some creases in the paper were beautifully reproduced.

* *Notices sur l'appareil de Ruhmkorff*, p. 46.

3. I now cut out of thin white letter-paper the word 'Volta,' and placed it between the plates of glass; they were submitted to electrisation as before, and the interior surface of one of them, without the paper letters, was subsequently exposed to the hydrofluoric acid vapour; the previously invisible figures came out perfectly, and formed a permanent and perfectly accurate etching of the word 'Volta,' as complete as if it had been done in the usual mode by an etching-ground. This, of course, could be washed and rubbed to any extent without alteration, and the results I have obtained give every promise for those who may pursue this as an art of producing very beautiful effects, enabling silhouette designs, or even fine engravings, to be copied on glass, &c.

4. I again electrised a plate in the same manner, and then covered the surface having the invisible image, with iodised collodion, and immersed it in a bath of nitrate of silver (40 grains to the ounce) in a room lighted by a candle, in the usual manner as for a photograph. It was then held opposite a window for a few seconds, and taken back into the darkened room, and on pouring over it a solution of pyrogallic acid the word 'Volta,' and the border of the glass beyond the limits of the tinfoil, were darkened, and came out with perfect distinctness, the other parts of the glass having been, as it were, protected by electrisation from the action of light; the figures were permanently fixed by a strong solution of hyposulphite of soda.

5. A similar experiment to the last was made, but after fixing the impression the collodion film was floated off; this contained the impression, as it does with an ordinary photograph; and the glass plates being washed with distilled water and dried, showed no impression when breathed on.

6. An electric impression of the word 'Volta' was well rubbed with a handkerchief, then washed with water and alcohol, then dried; the impression still came out by breathing upon it. Some one of the chemicals used in the collodion process had probably had the effect of removing the figure in Experiment 5, but I have not yet ascertained to which this removal is due.

7. Letters cut in tinfoil were substituted for those of

paper; the effect was the same, but it seemed to me more feeble.

8. A solution of nitrate of silver was poured over the surface of an electrised plate, so as to form a bath on its surface; a rake, formed of ten common pins, was made to touch the glass with its points along the course of the invisible image; the silver was of course precipitated in an arborescent form, and I thought it probable that the lines of deposition might follow the course of the invisible image, but I could not be certain of any such effect, though in one experiment there seemed to be some slight indication of it.

The above experiments were repeated many times, both with positive and negative electricity from the coil. At first I believed I had found a remarkable difference in the effect of the change of direction of the discharge in the cases where hydrofluoric acid was employed; and in two experiments, in which the tinfoil of the upper glass plate was connected with the positive terminal, the impression of the word on the lower side of the upper plate after exposure to the vapour was polished, while the residue was frosted; and in two subsequent cases, the upper tinfoil being connected with the negative terminal, the reverse was the case. In subsequent experiments, however, great irregularities took place in this relation, and it seemed to depend on the time of exposure and on slight differences in the distance between the paper letters and the glass, the latter not being brought into perfectly uniform contact with the surfaces of the glass.

After the first few experiments I placed a marble paperweight on the upper glass, and found the effects more uniform and perfect.

An electrisation for periods of from five to ten minutes produced the sharpest and clearest effects; when the electrisation was prolonged, a blur or second margin gradually appeared, and increased in extent around the outline of the letters, having somewhat the appearance which would have been presented had the paper letters been moist, and the liquid slightly extended itself from their edges over the glass.

When the electrisation was thus prolonged the figures were visible on inclining the glass to the light, without

breathing on them, and gave a strong impression of the glass having suffered a superficial disintegration or decomposition, but I could not by the microscope or by polarised light detect a difference in its structure. I hope, however, to do so by a more prolonged and varied examination.

It may be well to state that counter-experiments were made, such as allowing the plates to remain with the letters between them, but without electrisation, in which case no effect was produced; but it is probable, by analogy with Moser's class of phenomena, that a very long period of contact would produce some effect.

ON THE INFLUENCE OF LIGHT ON THE POLARISED ELECTRODE.*

Phil. Mag.,' December 1858.

Soon after the experiments of Daguerre were published it occurred to me that the galvanometer might be used as a test for the chemical effects of light; and I succeeded in obtaining a deflection of the needle by allowing a beam of light suddenly to impinge on a daguerreotype plate in a trough of water—the plate being connected with one extremity of a galvanometer, and a gridiron of silver wire placed in front of the plate with the other. This experiment I showed at a lecture at the London Institution in 1843; and it was subsequently used as an illustration of the convertibility of force, in my essay on the 'Correlation of Physical Forces.'

I tried some farther experiments at the time, without obtaining results of any importance, but, as galvanometers at that period had not reached the degree of delicacy they have since attained in the hands of M. Ruhmkorff, I determined this summer to resume the enquiry; and the results which I have obtained I now proceed to describe. The galvanometer used in the following experiments is by Ruhmkorff, formed of 544 feet of fine copper wire, and though not as delicate as

* Read at the meeting of the British Association, Leeds, Sept. 1858.

the very long-wired instruments used by M. DuBois Reymond and others, it has proved sufficiently delicate for most of the effects I aimed at.

The idea with which I started was to arrange two plates of platinum in an electrolyte in such manner that a bright beam of light should impinge on one while the other was in darkness, and yet to allow free electrolytic communication. After making a somewhat complex apparatus, which did not answer the purpose, the following simple means of effecting my object was adopted: In a cell similar to those used for the nitric acid battery the outer cell being of thin glass and the inner one of porous ware, two platinum plates were placed, each of six inches by two—four inches by two, or the immersed portions of the plates, being platinised or coated with a deposit of black platinum. Both the outer and inner cell were filled with distilled water slightly acidulated with sulphuric acid; and some tow steeped in the same solution was stuffed into the upper part of the porous cell around the platinum, so that this latter plate was perfectly excluded from light. The extremities of the two plates were metallically connected. A brass cylinder, covered at the top, was placed over the whole, its lower circumference resting on a circular pad of paper, so as to exclude light.

The apparatus, thus disposed, was set aside for ten days, so as to allow the local currents to subside. At the expiration of this period the apparatus was taken into bright sunlight, the position of the plates so arranged that the one in the outer or glass cell should be opposite the sun, the terminals connected with the galvanometer, and the temporary deflection occasioned by polarisation allowed to subside, or rather to reach a fixed point, for there was always a slight deflection.

The brass cylinder which excluded light from the apparatus was now removed; and the galvanometer needle instantly deviated to 10°, the platinum exposed to sunlight being positive to that in the dark, or as zinc to copper. The platinum plates were now reversed, that which had been in the outer cell placed in the porous cell, and *vice versâ*, and the apparatus again set aside for ten days; at the end of this period it was again taken out, the experiment repeated, and the same

result obtained; *i.e.* on removing the brass cylinder there was a deflection of 12°, the platinum exposed to light being positive to the sheltered one.

This identity of electrical effect taking place with the reversed plates seemed so strongly in favour of the impact of the solar rays having an initiatory effect in producing a voltaic current, that the only remaining point seemed to be to ascertain whether it was due to light or heat, to the chemical or calorific rays of the sun; yet the conclusion I then came to was erroneous, as will presently be seen.

In order to ascertain how far the effect was due to heat, I arranged, in a room lighted by a small candle, the same apparatus over a fire of asbetos heated by coal-gas, so that both radiant heat and an ascending current of hot air impinged on the side of the glass in which was the exposed platinum, while the opposite side was entirely sheltered from the heat by a metallic shelf, on which the cell rested; this experiment was continued until one side of the cell was uncomfortably hot to the hand, while the other side was quite cool; but not the slightest deviation of the galvanometer took place.

I now repeated the former experiment with sunlight, changing the liquid each time. In three successive experiments the deflections on the impact of light were in the same direction, the exposed platinum being positive; but in a fourth the deflection was in the reverse direction, the exposed platinum being negative. In several subsequent experiments there was always a notable deflection which ensued on the impact of light; but it was sometimes in one direction and sometimes in the other. I ultimately discovered that, in the deflection produced by light, the needle of the galvanometer deviated in the same direction which it took upon the first contact of the wires connected with the platinum plates. The effect of light was therefore to increase the deflection occasioned by the polarisation of the platinum plates; and this my subsequent experiments have, I think, fully established. Although the experiment on the impact of heat seemed to show that the heating effect of the solar rays was not the cause of the phenomena, yet it might well be that the solar rays absorbed by the platinum black, would produce a greater heating effect

at the actual point of contact of the platinum and liquid than any non-luminous heat would produce; and I was therefore anxious to ascertain whether the different coloured rays of light showed any difference in their effects. To this end I procured three plates of coloured glass, one blue, the second yellow, and the third red; a strip of thick brown paper was pasted to the opposite sides of each of these plates of glass, so as to form a nearly cylindrical chamber cut by the plane of the glass. A cover was placed over each of them; and the chambers so formed could be placed over the cell containing the platinum plates, the coloured glass plates intervening between the sun and the platinum in the outer cell. A great number of experiments were made with these apparatus; and in all the deviations of the galvanometer were notably greater with the blue glass than with the yellow or red, and, of the latter two, the yellow gave slightly greater deflections than the red glass.

This result is, I think, conclusive in favour of the effect being due to the chemical, not to the calorific rays of the sun, the more so when we consider that the yellow allowed a far larger quantity of light to pass than the blue glass. I may also add that I have obtained a slight galvanometric deflection when diffused daylight was allowed to impinge on the platinum plate, and when there was no perceptible difference of temperature between the illuminated and the non-illuminated plates.

The superiority of the yellow over the red was not so strongly marked; and, considering that the yellow glass allowed much more light to pass than the red, I am not disposed to think that there was any actual superiority in the former; the effects observed with these two colours are, however, corroborative of the effects not being due to the red or heating rays of the sun.

I substituted for the water acidulated with sulphuric acid (which may be regarded electrically as pure water with its conducting power improved), muriatic and nitric acids; the effects were the same, but less marked with the nitric acid, probably from its more completely depolarising the plates.

In a small number of experiments the following effect took

place: After a certain time of connexion with the galvanometer, the cover being over the apparatus, the sign of polarisation changed; *i.e.* supposing the needle of the galvanometer to have deviated to the left, and indicated that the exposed platinum was positive, the needle would gradually return, pass the zero-point, and be deviated to the right; when this was the case, on removing the cover, the effect occasioned by the impact of light was a return of the needle towards the zero-point, indicating an influence in the direction of the original polarisation. On setting aside the apparatus for twenty-four hours with the plates in metallic connection, and then repeating the experiment, the deviation of the galvanometer was in the direction of the final polarisation. This apparent anomaly may have arisen from a conflict of two classes of currents, the one arising from imperfect mixing or want of homogeneity in the liquid, and the other from the state of surface of the platinum; the latter would most probably be the current affected by light.

As the general effect of light was to increase the deflections occasioned by polarisation, whatever direction this assumed, it seemed probable that the exclusion of one of the plates from the light, which I had commenced with in the hope of obtaining currents initiated by light, was unnecessary, and that the observed effects would be rather increased by exposing both plates to light. I therefore arranged two platinised plates in a cell without a porous diaphragm, inclined to each other like the letter V, but without contact, and allowed the light to impinge on the interior surfaces, or those opposed to each other; but, to my surprise, the effect was very trifling, the needle deviating only one or two degrees, and that in a sluggish and irregular manner.

When, however, the two plates were arranged parallel, the one shading the other from the light, as good deflections were produced as with the porous cell—the more so if the back or shaded plate was of polished and the front plate of platinised platinum.

Why light should produce a greater augmentation of the current when impinging on one than on the two plates I cannot explain on the theory of polarized plates, and will leave for farther experiment.

In all the experiments I have made on the subject of this paper the most marked effect upon the galvanometer is produced when the polarisation causes a small permanent deflection of from 5° to 10° ; when the polarisation of the plates is extremely slight, the effect of light is very feeble ; and when the polarisation is considerable, so as to deflect the galvanometer to 20° or 30°, the increased force required to produce a small increase of deflection is too great to afford notable results.

I have used the term polarisation, having no better word to indicate the feeble currents which are always observed when two platinum plates immersed in a liquid are connected with a delicate galvanometer. The electrical currents which would ensue if the plates were polarised by connecting them with a voltaic battery and then detaching them, would be far too powerful for the delicate indications which I have been examining. There can be no doubt, at least to those who adopt the chemical theory of the voltaic pile, that both these classes of polarisation are due, when one homogeneous liquid is employed, to slight deposits on the plates, either of films of gas or of some substance which acts chemically on the liquids, and the effect of light would seem simply to be an augmentation of the chemical action taking place at the surface of the electrodes, which is the *locus* where the chemical changes producing or produced by voltaic currents are always observable.

With more sensitive galvanometers, and with a greater variety of solutions, this class of experiments may, I venture to hope, be found important in farther investigating the effects of light on chemical actions ; and the pure coloured rays of the spectrum may be employed.

There can, I think, from analogy, be little doubt that light would influence those actions of surface which are comprehended among the various effects to which the term catalysis is applied. In an experiment I made in the month of September 1851 two similar glass tubes, containing each 15 grains of water, were placed, the one under an opaque porcelain, and the other under a glass vessel of the same size, with capsules of sulphuric acid by their sides ; I found that evaporation took place much more rapidly in the one exposed to light,

though it was in a room with a northern aspect, on which the sun never shone. In twelve days the water under the glass vessel had lost 6·3 grains, that under the porcelain 5·4, showing a difference of nearly a sixth part in the evaporation in favour of the tube exposed to light. I mention this experiment here as showing a probability that the liberation of vapour or gas may be accelerated by light, as M. Donny's remarkable experiments seem to show that evaporation is a surface action; and the effect of light on polarised plates may be somewhat of the same nature. I will not, however, enter into theories, which must be necessarily vague, on such a novel subject, but conclude by giving a table of some of the most trustworthy results which I have obtained when the sunlight has been most steady, in order to show the extent of deflections and differences with coloured light: they were all made between the hours of 10 a.m. and 12, and on the finest days I could select. My first experiments were made in London, in the months of June and July. I was then absent for some time on circuit; and I have resumed and continued them during my vacation in the months of August and September.

June 21, apparatus having been arranged on June 12. —Solution, water with a few drops of sulphuric acid.

Deflection by sunlight 8°, *i.e.* from 12° the deflection by polarisation, to 20°.

Exposed platinum positive.

July 4.—Same solution, platinum having been changed on the 24th.

Deflection by sunlight of 9°, i.e. from 1° to 10°.

Exposed platinum positive.

August 24.—Solution, water with sulphuric acid.

	°
Deflection by polarisation	1
Sunlight	5
Blue glass	3
Yellow glass	2
Red glass	2

Exposed platinum positive.

August 25.—Solution, water with sulphuric acid.

	°
Deflection by polarisation	2
Sunlight	7
Blue glass	4·5
Yellow glass	3
Red glass	3

Exposed platinum positive.*

September 4.—Very dilute muriatic acid.

	°
Deflection by polarisation	6
Sunlight	14
Blue glass	9
Yellow glass	7·5
Red glass	7·5
Diffuse daylight	8

Exposed platinum negative.

September 6.—Very dilute nitric acid.

	°
Deflection by polarisation	1
Sunlight	7
Blue glass	3
Yellow glass	2
Red glass	1

Exposed platinum negative.

September 8.—Dilute nitric acid.

	°
Deflection by polarisation	3
Sunlight	6

Exposed platinum negative.

* In a second cell with the same solution the exposed plate was negative, and the deflection increased by sunlight in the same direction as the polarisation, from 2° to 10°.

Dilute muriatic acid.

	°
Deflection by polarisation	4
Sunlight	10
Blue glass	7
Yellow glass	6
Red glass	5·5

Exposed platinum negative.

September 10.—Dilute nitric acid.

	°
Deflection by polarisation	1
Sunlight	5

Exposed platinum positive.

Dilute muriatic acid. Polished platinum plates.

	°
Deflection by polarisation	1
Sunlight	2

Exposed platinum negative.

September 14.—Two sets, one of polished, the other of platinised platinum plates, in dilute sulphuric acid, without porous diaphragm.

The polished plates gave :—

	°
Deflection by polarisation	2
With sunlight.	3

Platinised plates gave :—

	°
By polarisation	3
With sunlight	4

The pair with black platinum was then tried with a porous cell ; and as it was not set aside for more than a few minutes, to allow the polarisation to subside, the

	°
Deflection by polarisation was	15
Increased by sunlight to	20

Exposed platinum positive.

This being covered, but kept in connection with the galvanometer, the polarisation gradually diminished, passed the zero-point, and took up an opposite deflection of 7·5°; the outer platinum being now negative, the effect of sunlight was now to reduce the

	°
Deflection to	3
Red glass to	6·5
Yellow glass to	6·5
Blue glass to	5·5

On September 16, this same cell having been set aside in the interim, with the plates metallically connected, the deflection by polarisation was increased by sunlight, the exposed platinum being negative.

September 15.—Black and polished platinum plates in glass cell without porous cell, the black platinum being towards the light, and the plain platinum shaded by it; the black platinum was positive to the plain :—

	°
Deflection by polarisation	10
Sunlight	18
Blue glass	12
Yellow glass	11

After half an hour in connexion with a galvanometer :—

	°
Deflection by polarisation had sunk to	4
Sunlight	12
Blue glass	6
Yellow glass	4
Red glass	4

ON THE TRANSMISSION OF ELECTROLYSIS ACROSS GLASS.

Phil. Mag., Aug. 1860. Read at the Chemical Section of the British Association, June 28, 1860.

If glass or an equally non-conducting substance be interposed between electrodes in an electrolyte, so that there be no liquid

communication around the edges, it is hardly necessary to say that, according to received opinions and experiments, no current passes and no electrical effects appear. I was led by some theoretic considerations to think that this rule might not be without an exception, and the following experiment realised my views :—

A Florence flask, well cleaned and dried, was filled two-thirds full of distilled water, with a few drops of sulphuric acid added to it, and placed in an outer vessel containing similar acidulated water, which reached to the same height as the liquid in the interior. A platinum wire was passed through a glass tube, one end of which was hermetically sealed to the platinum, but so as to allow a small portion of the wire (about $\frac{1}{20}$th of an inch) to project; the other end of the wire was passed through a cork, and the cork fitted to the mouth of the flask; when the cork was introduced the projecting end of the platinum wire was three-quarters of an inch below the surface of the interior liquid; a similar coated wire was dipped into the outer liquid, and this and the wire which passed through the cork were brought respectively into connection with the extremities of the secondary coil of a Ruhmkorff apparatus. Upon the latter being excited by the battery a stream of minute bubbles arose from both the platinum points, proving clearly that electrolysis took place, notwithstanding the interposition of the glass. The portions of the flask above the liquid, both outside and inside, were perfectly dry, so that there could have been no communication of the current over the surface of the glass. This was farther proved by removing the outer wire a short distance from the surface of the water, when sparks passed nearly equal in length to those which took place between wires from the terminals. As the outer wire was farther removed, keeping it near the glass, the sparks passed along the surface of the latter for a short distance; and as it was farther removed they ceased, thus showing conclusively that there was no passage of electricity over the upper and unwetted surface of the glass.

With distilled water unacidulated I could observe no effect of electrolysis.

With acidulated water and the same arrangement I could

detect no signs of electrolysis, when instead of the Ruhmkorff coil a nitric acid battery of thirty cells was employed.

In the first experiment the evolution of gas gradually diminished, and ceased after about twenty minutes' experiment; but upon intercepting communication with the battery for ten minutes, and then re-connecting it, the evolution took place again; or a recurrence of electrolysis could be produced by reversing the direction of the current.

When the flask, after twenty minutes' experiment, was removed from the outer vessel and tapped, minute bubbles rose from the interior surface. When a tolerably thick test-tube was used instead of the Florence flask, a very slight effect of electrolysis could be detected; and when the outer wire was removed to a short distance from the surface, sparks passed, but not of half the length of those with the Florence flask. When, however, a large phial of somewhat greater contents than the Florence flask was used, the effects were the same as with the latter, showing, as I expected, that surface is an important element in the success of the experiment.

There seems little doubt from the above experiments that the electrolysis was effected by induction across the thin glass of the Florence flask; and its cessation after a time and recurrence after interruption of the current would seem to indicate something like a state of charge or of polarisation of the surface of the glass.

Whether the bubbles which arose from the interior surface of the glass were the effect of electrolytic action or mere air-bubbles, cannot be affirmed with certainty; but as there was distinct evolution from the platinum wires, the corresponding elements must have been either dissolved, evolved, or deposited somewhere, and the most probable place of evolution would be the surface of the glass. If this be so, the glass would act in effect just as an interposed plate of inoxidable metal, though the one acts by induction, the other by conduction.

The oxygen and hydrogen may, however, be spread over the surface of the glass without evolution in the form of gas;

and when the glass is, so to speak, coated by the elements, decomposition would cease.

The quantity of gas in the above experiment was too small for analysis; and in all probability, could it have been examined, some mixed gas would have been found to have been eliminated from each electrode. When, however, there is a small interruption in the secondary circuit of Ruhmkorff's coil, producing a rapid succession of sparks, I have found that between electrodes in the same circuit true polar decomposition takes place, and a galvanometer is steadily deflected according to the direction of the current, while without such interruption the movements of its needle are most irregular. I therefore repeated the experiment of interposed glass with an interruption in the circuit, and found that electrolysis took place apparently as before; and in this case there was probably true polar decomposition.

Although I failed with thirty cells of the nitric acid battery, I should expect that a battery of very high intensity, such as the 500 cells nitric acid, or the water-battery of Mr. Gassiot, would produce effects of electrolysis across glass without the use of the coil.

AN EXPERIMENT IN MAGNETO-ELECTRIC INDUCTION.

Philosophical Magazine, March 1868.

SHORTLY after the publication of Mr. Wilde's experiments on magneto-electric induction, it occurred to me that some of the ordinary effects of the Ruhmkorff coil might be produced by applying to it a magneto-electric machine. I tried an ordinary medical machine with a small coil, made by Mr. Apps, of $3\frac{1}{2}$ inches length by 2 inches diameter, and having about $\frac{3}{4}$ of a mile of fine secondary wire.

The result was very unexpected. The terminals of the magneto-electric coils being connected with the primary coil of the Ruhmkorff, and the contact-breaker being kept closed,

so as to make a completed circuit of the primary wire (a condition which would have appeared *à priori* essential to success), no effect was produced; while if the circuit was interrupted by keeping the contact-breaker open, sparks of 0·3 of an inch passed between the terminals of the secondary coil of the Ruhmkorff, and vacuum-tubes were readily illuminated. Here there was in effect no primary coil, no metallic connexion for the primary current; and yet a notable effect was produced.

I did not at the time publish this experiment farther than by communicating it to a few friends, hoping to be able to find a satisfactory explanation of it. All I have observed since is that the effect is dependent upon the condenser, for when that is removed no result is produced.

It would appear, then, to depend on an electrical wave or impulse, shot, so to speak, into the uncompleted primary coil, similar to the wave which will deflect in succession magnetic needles placed at different distances on a telegraphic cable, without the current passing through the whole length of wire, as shown in the experiments of Mr. Latimer Clark and others. But why there should be no effect, or an inappreciable one, when the primary circuit is completed, the current being alternated by the rotation of the coils of the magneto-electric machine, I cannot satisfactorily explain.

ON SOME EFFECTS OF HEAT ON FLUIDS.

Journal of the Chemical Society, May 21, 1863.

HAVING been honoured by an invitation from the Council of the Chemical Society to give a lecture on one of the evening meetings of the Society, it was not without hesitation that I accepted the task. My professional occupation having of late years so much prevented any continuous devotion of mind to scientific investigation, it was only by reverting to researches made several years ago that there was a chance of my communicating anything worthy the attention of the Society. I

had, however, at one period made a rather careful investigation into some phenomena, suggested partly by a paper of M. Donny's, published in 1843, and partly by my own experiments on the decomposition of water by heat ; and although this investigation did not lead to results as important as I at the time anticipated, yet, as they seemed to add something to our stock of knowledge, and may, it is hoped, form a starting-point for important discoveries, I have ventured to bring them forward on this occasion.

The paper of M. Donny (' Mémoires de l'Académie Royale de Bruxelles,' 1843) makes known the fact that in proportion as water is deprived of air, the character of its ebullition changes, becoming more and more abrupt, and boiling like sulphuric acid with *soubresauts*, and that between each burst of vapour the water reaches a temperature above its boiling-point. To effect this, it is necessary that the water be boiled in a tube with a narrow orifice, through which the vapour issues ; if it be boiled in an open vessel, it continually re-absorbs air and boils in the ordinary way.

In my experiments on the decomposition of water by heat I found that with the oxyhydrogen gas given off from ignited platinum plunged into water, there was always a greater or less quantity of nitrogen mixed ; this I could never entirely get rid of, and I was thus led into a more careful examination of the phenomenon of boiling water, and set before myself this problem—what will be the effect of heat on water perfectly deprived of air or gas ?

Two copper wires were placed parallel to each other through the neck of a Florence flask, so as nearly to touch the bottom ; joining the lower ends of these was a fine platinum wire, $1\frac{1}{2}$ inch long, and bent horizontally into a curve. Distilled water, which had been well boiled, and cooled under the receiver of an air-pump, was poured into this flask, so as to fill about one-fourth of its capacity. It was then placed under the receiver of an air-pump, and one of the copper wires brought in contact with a metallic plate covering the receiver, the other bent backwards over the neck of the flask, and its end made to rest on the pump plate. By this means, when the terminal wires from a voltaic battery

were made to touch, respectively the upper and the lower plate, the platinum wire would be heated, and the boiling continued indefinitely in the vacuum of a very excellent air-pump. The effect was very curious: the water did not boil, but at intervals a burst of vapour took place, dashing the water against the sides of the flask, some escaping into the receiver. (There was a projection at the central orifice of the pump plate, to prevent this overflow getting into the exhausting tube.)

After each sudden burst of vapour the water became perfectly tranquil, without a symptom of ebullition until the next burst took place. These sudden bursts occurred at measured intervals, so nearly equal in time that, had it not been for the escape from the flask at each burst of a certain portion of water, the apparatus might have served as a timepiece.

I showed this experiment at one of my lectures at the time, as affording an illustration of the action of the geysers, of which it seemed to me to afford a rational explanation. Supposing a source of heat at a certain depth beneath the earth's surface, and subterranean wells whose only communication with the air was a narrow tube, probably formed by the issue of vapour, the air, if any, dissolved in the water of these wells would be expelled, and the boiling would take place at intervals and by sudden bursts, instead of in the ordinary way.

This experiment, though instructive, did not definitively answer the question I had proposed, as I could not of course ascertain whether there was some minute residuum of gas which would form the nucleus for each ebullition, and I proceeded with others. A tube of glass, five feet long, and $\frac{4}{10}$ths inch internal diameter, was bent into a V shape; into one end a loop of platinum wire was hermetically sealed with great care, and the portion of it in the interior of the tube was platinised. When the tube had been well washed, distilled water, which had been purged of air as before, was poured into it to the depth of eight inches, and the rest of the tube filled with olive-oil; when the V was inverted the open end of the tube was placed in a vessel of olive-oil, so that there would be eight inches of water resting on the platinum wire, separated

from the external air by a column of four feet four inches of oil. The projecting extremities of the platinum wire were now connected with the terminals of a voltaic battery and the water heated by the wire ; some air was freed, and ascended to the level of the tube—this was made to escape by carefully inverting the tube, so as not to let the oil mix with the water, and the experiment continued. After a certain time the boiling assumed an uniform character, not by such sudden bursts as in the Florence flask experiment, but with larger and more distinct bursts of ebullition than in its first boiling.

The object of platinising the wire was to present more points for the ebullition, and to prevent *soubresauts* as much as possible.

The experiment was continued for many hours, and, in some repetitions of it, for days. After the boiling had assumed a uniform character the progress of the vapour was carefully watched, and as each burst of vapour condensed in the oil, which was kept cool, it left a minute bead of gas, which ascended through the oil to the bend of the tube : a bubble was gradually formed here which did not seem at all absorbed by the oil. This was analysed by a eudiometer, which I will presently describe, and proved to be nitrogen. The beads of gas, when viewed through a lens and micrometer scale at the same height in the tube, appeared as nearly as may be of the same size. No bubble of vapour was condensed completely, or without leaving this residual bubble. The experiment was frequently repeated, and continued until the water was so nearly boiled away, that the oil, when disturbed by the boiling, nearly touched the platinum wire ; here it was necessarily stopped.

To avoid any question about the boiling being by electrical means, similar experiments were made with a tube, without a platinum wire, closed at its extremity, and the boiling was produced by a spirit-lamp. The effects were the same, but the experiment was more difficult and imperfect, as the bursts of vapour were more sudden, and the duration of the intervals more irregular.

The beads of gas were extremely minute, just visible to the naked eye. I cannot find any record of their exact measure.

In these experiments there was no pure boiling of water, *i.e.* no rupture of cohesion of the molecules of water itself, but the water was boiled, to use M. Donny's expression, by evaporation against a surface of gas.

It is hardly conceivable that air could penetrate through such a column of oil, the more so as the oil did not perceptibly absorb the nitrogen freed by the boiling water and resting in the bend of the tube; but to meet this conjectural difficulty, the following experiment was made: A tube, one foot long and $\frac{2}{10}$ths inch internal diameter, bent into a slight obtuse angle, had a bulb of $\frac{3}{4}$ inch diameter blown on it at the angle; this angle was about three inches from one end and nine from the other; a loop of platinum wire was sealed into the shorter leg, and the whole tube and bulb filled with and immersed into mercury; water, distilled and purged of air as before, was allowed to fill the short leg, and by carefully adjusting the inclination, the water could be boiled so as to allow bubbles to ascend into the bulb and displace the mercury. The effect was the same as with the oil experiment, no ebullition without leaving a bead of gas, the gas collected in the bulb, and was cut off by what may be termed a valve of mercury, from the boiling water, then allowed to escape, and so on; the experiment was continued for many days, and the bubbles analysed from time to time; they proved, as before, to be nitrogen, and, as before, continued indefinitely.

A similar experiment was made without the platinum wire, and though, from the greater difficulties, the experiment was not so satisfactory, the result was the same.

As the mercury of the common barometer will keep air out of its vacuum for years, if not for centuries, there could be no absorption here from the external atmosphere, and I think I am fairly entitled to conclude from the above experiments—which I believe went far beyond any that have been recorded—that no one has yet seen the phenomenon of pure water boiling, *i.e.* of the disruption of the liquid particles of the oxyhydrogen compound, so as to produce vapour which will, when condensed, become water, leaving no permanent gas. Possibly, in my experiment of the decomposition of

water by ignited platinum, it may be that the sudden application of intense heat, and in some quantity, so forces asunder the molecules that, not having sufficient nitrogen dissolved to supply them with a nucleus for evaporation, the integral molecules are severed, and decomposition takes place. If this be so, and it seems to me by no means a far-fetched theory, there is probably no such thing as boiling, properly so called, and the effect of heat on liquids in which there is no dissolved gas may be to decompose them.

Considerations such as these led me to try the effect of boiling on an elementary liquid, and bromine occurred as the most promising one to work upon. As bromine could not be boiled in contact with water, oil, or mercury, the following plan was ultimately devised: A tube, four feet long and $\frac{4}{10}$ths inch diameter, had a platinum loop sealed into one closed extremity; bromine was poured into the tube to the height of four inches; the open end of the tube was then drawn out to a fine point by the blow-pipe, leaving a small orifice; the bromine was then heated by a spirit-lamp; and when all the air was expelled, and a jet of bromine vapour issued from the point of the tube, it was sealed by the blow-pipe. There was then, when the bromine vapour had condensed, a vacuum in the tube above the bromine. The platinum loop was now heated by a voltaic battery and the bromine boiled; this was continued for some time, care being taken that the boiling should not be too violent. At the end of a certain period—from half-an-hour to an hour—the platinum loop gave way, being corroded by the bromine: the quantity of this had slightly decreased. On breaking off, under water, the point of the tube, the water mounted and showed a notable quantity of permanent gas, which on analysis proved to be pure oxygen. As much as a quarter of a cubic inch was collected at one experiment. The platinum wire, which had severed at the middle, was covered with a slight black crust, which, suspecting it to be carbon, I ignited by a voltaic spark in oxygen in a small tube over lime-water; it seemed to give a slight opalescence to the liquid, but the quantity was so small that the experiment was not to be relied on. No definite change was perceptible in the bromine; it seemed to be a little darker in

colour, and had a few black specks floating in it, which I judged to be minute portions of the same crust which had formed on the platinum wire, and which had become detached.

The experiment was repeated with chloride of iodine, and with the same result, except that the quantity of oxygen was greater; I collected as much as half a cubic inch in some experiments, from an equal quantity of chloride of iodine; the platinum wire, however, was more quickly acted on than with the bromine, and also to some extent the glass of the tube around it.

Melted phosphorus was exposed to the heat of the voltaic disruptive discharge by taking this between platinum points in a tube of phosphorus, similarly to an experiment of Davy's, but with better means of experimenting; a considerable quantity of phosphuretted hydrogen was given off, amounting in several experiments to more than a cubic inch.

A similar experiment was made with melted sulphur and sulphuretted hydrogen given off, but not in such quantities as the phosphuretted hydrogen. I tried in vain to carry on these experiments beyond a certain point; the substance became pasty, mixed with platinum from the arc, and from the difficulty of working with the same freedom as when they were fresh, the glass tubes were always broken after a certain time. Had I time for working on the subject now, I should use the discharge from the Ruhmkorff coil, which had not been invented at the period of these experiments. At a subsequent period, when this discharge was taken in the vacuous receiver of an air-pump from a metallic point to a metallic capsule containing phosphorus, a considerable yellow deposit lined the receiver, which, on testing, turned out to be allotropic phosphorus. No gas is, however, given off. I had an air-pump (described 'Phil. Trans.' 1852, p. 101) which enabled me to detect very small quantities of gas, but I could get none. It was in making these experiments that I first detected the striæ in the electric discharge, which have since become a subject of such interesting observations, which are seen, perhaps, more beautifully in this phosphorus vapour than in any other medium, and which cease, or become very feeble, where the allotropic phosphorus is not produced.

I tried also phosphorus highly heated by a burning-glass in an atmosphere of nitrogen, but could eliminate no perceptible quantity of gas, though the phosphorus was changed into the allotropic form.

It is not difficult to understand why gas is not perceptibly eliminated in the last two experiments; the effect is probably similar to that described in my paper on 'The Decomposition of Water by Heat,' where, when the arc or electric spark is taken in aqueous vapour, a minute bubble of oxyhydrogen gas is freed and disseminated through the vapour, recombination being probably prevented by this dilution; but, however long the experiment may be continued, no increased quantity of the gas is obtained, all beyond this minute quantity being recombined. If, however, the bubble of gas be collected, by allowing the vapour to cool, and then expelled, a fresh portion is decomposed, and so on.

So with the phosphorus in the experiments in the air-pump and with the burning-glass, if any gas is liberated it is probably immediately recombined with the phosphorus; possibly a minute residuum might escape recombination, but the circumstances of the experiment did not admit of this being collected, as was the case with the gas in aqueous vapour.

When, on the other hand, the gas freed is immediately cut off from the source of heat, as when the spark is taken in liquids, an indefinite quantity can be obtained.

Decomposition and the elimination of gas may thus take place by the application of intense heat to a point in a liquid, or also in gas or vapours, but in the latter case it is more likely to be masked by the quantity of gas or vapour through which it is disseminated.

I believe there are very few gases in which some alteration does not take place by the application of the intense heat of the voltaic arc or electric spark. If the arc be taken between platinum points in dry oxygen-gas over mercury, the gas diminishes indefinitely, until the mercury rises, and by reaching the point where the arc takes place puts an end to the experiment. I have caused as much as a cubic inch of oxygen to disappear by this means. I at one time thought this was due to the oxidation of the platinum, but the high heat renders

this improbable, and the deposit formed on the interior of the glass tube in which the experiment is made has all the properties of platinum-black; so if the spark from a Ruhmkorff coil be taken in the vapour of water for several days, a portion of gas is freed which is pure hydrogen, the oxygen freed being probably changed into ozone and combined with the mercury or dissolved by the water.

I have alluded to the eudiometer by which I analysed the gases obtained in these experiments; it was formed simply of a tube of glass frequently not above $2\frac{1}{2}$ millemetres in diameter, with a loop of wire hermetically sealed into one end, the other having an open bell-mouth. By a platinum wire a small bubble of the gas to be examined could be got up through water or mercury into the closed end of the tube, and by the addition of a bubble of oxygen or hydrogen gas a very accurate analysis of very minute quantities of gas could be made; I have analysed by this means quantities no larger than a partridge-shot.

I need hardly allude to results on the compound liquids, such as oils and hydrocarbons, as the fact that permanent gas is given off in boiling such liquids would not be unexpected; but the above experiments seem to show that boiling is by no means necessarily the phenomenon that has generally been supposed, viz. a separation of cohesion in the molecules of a liquid from distension by heat. I believe, from the close investigation I made into the subject, that (except with the metals, on which there is no evidence) no one has seen the phenomenon of pure boiling without permanent gas being freed, and that what is ordinarily termed boiling arises from the extrication of a bubble of permanent gas, either by chemical decomposition of the liquid, or by the separation of some gas associated in minute quantity with the liquid, and from which human means have hitherto failed to purge it; this bubble once extricated, the vapour of the liquid expands it, or, to use the appropriate phrase of M. Donny, the liquid evaporates against the surface of the gas.

My experiments are, in a certain sense, the complement of his. He showed that the temperature of the boiling-point was raised in some proportion as water was deprived of air,

and that under such circumstances, the boiling took place by *soubresauts*. I have, I trust, shown that when the vapour liberated by boiling is allowed to condense, it does not altogether collapse into a liquid, but leaves a residual bubble of permanent gas, and that at a certain point this evolution becomes uniform.

Boiling, then, is not the result of merely raising a liquid to a given temperature, it is something much more complex.

One might suppose that with a compound liquid the initial bubble by which evaporation is enabled to take place might, if all foreign gas were or could be extracted, be formed by decomposition of the liquid, but this could not be the case with an elementary liquid—whence then comes the oxygen from bromine or the hydrogen from phosphorus and sulphur? —as with the nitrogen in water, it may be that a minute portion of oxygen, hydrogen, or of water is inseparable from these substances, and that if boiled away to absolute dryness, a minute portion of gas would be left for each ebullition.

With water there seems a point at which the temperature of ebullition and the quantity of nitrogen yielded become uniform, though the latter is excessively minute.

The circumstances of the experiments with bromine, phosphorus, and sulphur, did not permit me to push the experiment so far as was done with water, but, as far as it went, the result was similar.

When an intense heat, such as that from the electric spark or voltaic arc, is applied to permanent gas, there are, in the greater number of cases, signs either of chemical decomposition or of molecular change; thus compound gases, such as hydrocarbons, ammonia, the oxides of nitrogen, and many others are decomposed. Phosphorus in vapour is changed to allotropic phosphorus, oxygen to ozone, which, according to present experience, may be viewed as allotropic oxygen; there may be many cases where, as with aqueous vapour, a small portion only is decomposed, and this may be so masked by the volume of undecomposed gas as to escape detection; if, for instance, the vapour of water were incondensable, the fact that a portion of it is decomposed by the electric spark or ignited platinum would not have been observed.

All these facts show that the effect of intense heat applied to liquids and gases is far more complex, and presents greater interest to the chemist, than has generally been supposed. In far the greater number of cases, possibly in all, it is not mere expansion into vapour which is produced by intense heat, but there is a chemical or molecular change. Had circumstances permitted I should have carried these experiments farther, and endeavoured to find an *experimentum crucis* on the subject; there are difficulties with such substances as bromine, phosphorus, &c., arising from their action on the substances used to contain and heat them, which are not easy to vanquish, and those who may feel inclined to repeat my experiments will find these difficulties greater than they appear in narration; but I do not think they are insuperable, and hope that, in the hands of those who are fortunate enough to have time at their disposal they may be overcome.

To completely isolate a substance from the surrounding air and yet be able to experiment on it, is far more difficult than is generally supposed. The air-pump is but a rude instrument for such experiments as are here detailed.

Caoutchouc joints are out of the question; even platinum wires carefully sealed into glass, though, as far as I have been able to observe, forming a joint which will not allow gas to pass, yet it is one through which liquids will effect a passage, at all events when the wires are repeatedly heated.

In some experiments with the ignited platinum wire hermetically sealed into a tube of glass, the end of the tube containing the platinum wire was placed in a larger tube of oil to lessen the risk of cracking the glass. After some days' experimenting, though the sealing remained perfect, a slight portion of carbon was found in the interior liquid. This does not affect the results of my experiments, as I repeated them with glass tubes closed at the end and without platinum wires, and also without the oil-bath, but it shows how difficult it is to exclude sources of error. When water has been deprived of air to the greatest practicable extent it becomes very avid for air. The following experiment is an instance of this: A single pair of the gas-battery, the liquid in which was cut off from the external air by a greased glass stopper, had one

tube filled with water, the other with hydrogen, the platinised platinum plates in each of these tubes were connected with a galvanometer, and a deflection took place from the reaction of the hydrogen on the air dissolved in the water. After a time the deflection abated, and the needle returned to zero, all the oxygen of the air having become combined with the hydrogen. If now the stopper were taken out, a deflection of the galvanometric needle immediately took place, showing that the air rapidly enters the water, as water would a sponge. Absolute chemical purity in the ingredients is a matter for refined experiments, almost unattainable; the more delicate the test, the more some minute residual product is detected; it would seem (to put the proposition in a somewhat exaggerated form) that in nature everything is to be found in anything if we carefully look for it.

I have indicated the above sources of error to show the close pursuit that is necessary when looking for these minute residual phenomena. Enough has, I trust, been shown in the above experiments to lead to the conclusion that, hitherto, simple boiling, in the sense of a liquid being expanded by heat into its vapour without being decomposed or having permanent gas eliminated from it, is a thing unknown. Whether such boiling can take place may be regarded as an open question, though I incline to think it cannot; that if water, for instance, could be absolutely deprived of nitrogen, it would not boil until some portion of it was decomposed; that the physical severance of the molecules by heat is also a chemical severance. If there be anything in this theoretic view, there is great promise of important results on elementary liquids, if the difficulties to which I have alluded can be got over.

The constant appearance of nitrogen in water, when boiled off out of contact with the air almost to the last drop, is a matter well worthy of investigation. I will not speculate on what possible chemical connexion there may be between air and water; the preponderance of these two substances on the surface of our planet, and the probability that nitrogen is not the inert diluent that is generally supposed, might give rise to not irrational conjectures on some unknown bond between air and water. But it would be rash to announce any theory

on such a subject; better to test any guess one may make, by experiment, than to mislead by theory without sufficient data, or to lessen the value of facts by connecting them with erroneous hypotheses.

ON APLANATIC TELESCOPES.

Phil. Mag., March 1867.

In my address as President of the British Association at Nottingham, last August, I suggested 'oily or resinous substances, such as castor-oil, Canada balsam, &c.,' as materials to be used, in combination with glass lenses, to reduce or annihilate the defect in the achromatic telescope arising from the irrationality of spectra. Since the delivery of that address, the specification of a patent of Mr. Wray has been published, in which these, among other substances of similar optical character, the principal one being oil of cassia, are named for the like purpose. Although provisional protection was obtained previously, the public could know nothing of the invention until the publication of the specification: this was shown to me by a friend last week.

In it the author does not, to my mind, get rid of the greatest difficulty which is experienced in the use of such substances, viz. the want of permanency in the telescope, arising from the unequal shrinking or drying of the substances when used between lenses not having touching curves. He says that the glasses, when the edges are chamfered, are hermetically sealed by the correcting substance; but I think he will find that this will not bear the test of time. It seems to me desirable that I should publish experiments I have made on this subject at different periods during many years, and which I have communicated to several persons, among whom I may name Dr. Frankland and Mr. Cooke, the well-known optician. Such publication may assist others in promoting this important object. Though at work on the subject for some time previously, the first published record on the subject I can lay my hand on is in the Monthly Astronomical

Notices for January 14, 1853, in which occurs the following paragraph: 'In some cases, where the inner curves of the flint- and crown-glasses approximate, Mr. Grove had employed with success a highly refracting cement made of very clear rosin and castor-oil, which, acting as a third lens or convex meniscus of a medium dispersing the coloured spaces differently from the other two lenses, corrected to a very great degree the chromatic without increasing the spherical aberration.'

Before the date of the above communication I had used for a similar purpose copal varnish between the lenses; and a three-inch object-glass, of four feet focus, so corrected by me was used for several years by Mr. B. Hill, of Swansea. Scarcely any secondary colour was perceptible; but in time, beginning at the second year, small bubbles appeared between the lenses, arising from the drying of the varnish, though the meniscus of it was so thin that it only shortened the focus 0·7 of an inch. It was this defect that led me to use the rosin and castor-oil, which formed a tough cement practically unalterable. The telescope alluded to in the above extract from the Monthly Notices was of three inches aperture and forty inches focus, the focus without the cement being forty-two inches; this telescope was a very good one: it divided γ Leonis, γ Virginis, and other stars of that class; the secondary spectrum was extremely slight, and capable of being detected only by a practised eye on a bright spot, such us the planet Venus. I used it for some time; but wishing to make it more perfect, I broke it, as I have done many others. I was convinced from the above and other results that oils or liquids which dry or shrink more or less in time, were open to objection similarly to Blair's object-glasses, where the correction is by a concave lens of liquid instead of a convex meniscus, and the substances for correction used with glass are therefore necessarily chosen from towards the bottom instead of the top of Brewster's list ('Optics,' Lardner's Cycl., pp. 374 and 375), in which oil of cassia stands first and sulphuric acid last. I was satisfied that the substance to be aimed at should be one which liquefied by heat and became solid when cold; Canada balsam in its ordinary state was therefore objectionable, but when

obtained hard and then tempered by admixture with castor-oil, so as to form a substance just taking, when cold, the impression of the nail, it succeeded very well, though, from my experience of the accidents with large glasses, I only tried it on small ones, two inches and under. Viscous Canada balsam and castor-oil, mixed and heated and cooled repeatedly, became ultimately solid.

I have nothing better to propose as a substance actually experimented on than the above two cements, viz. rosin and castor-oil, and fused Canada balsam and castor-oil. Oil of cassia I abandoned after an experiment or two, on account of its limpidity. Doubtless this and other substances might be used in combination with a fused resinous substance, but I have not tried it. Sulphuret of carbon, the third liquid in Brewster's list, mentioned by Mr. Wray, seems to me wholly impracticable, from its great volatility.

Although the substances at the head of Brewster's list, speaking generally, have a greater dispersive action on the more refrangible rays of the spectrum, and those at the bottom of it on the less refrangible rays (see Brewster's diagram of oil of cassia and sulphuric acid spectra, 'Optics,' p. 78), yet each substance has peculiarities, and must be experimentally tried by the old plan of two prisms or similar methods before it is used. None in theory absolutely correct the irrationality; but several do so practically, and the problem is for each specimen of flint and crown glass used to select a substance which sufficiently corrects the secondary spectrum and can be used by heat-fusion, so as to form a tough solid when cold. To this point, in my opinion the most important, Mr. Wray does not allude. Of liquids I have found castor-oil the most durable; and if the lenses be in an iron well-fitting cell (for it acts on brass rapidly), I am not sure that such material might not be used alone with advantage; but with large object-glasses it is excessively difficult to prevent the meniscus of castor-oil from being wedge-shaped, and so giving a tail to stars and planets. When the size of the glasses is notable it is extremely difficult to remedy this defect. I proceed to mention some modes of overcoming difficulties of manipulation which cost me much labour; I omit my failures.

1st. To apply solid transparent cements which melt by heat to object-glasses without danger of breaking or unannealing the glass.—An iron plate, about one-quarter of an inch in thickness, and at least an inch greater in diameter than the glass, is placed on a ring of metal and well levelled; on it place a piece of soft filtering-paper, and on this the object-glass, the flint glass being next the paper. Have the cement melted in a porcelain vessel placed in a sand-bath, the temperature being just high enough to fully melt it but not to carbonise it, and keep it melted as short a time as practicable; then heat uniformly, by a spirit-lamp underneath the iron plate, until the object-glass above is so hot that the hand can just bear it; remove the crown glass, pour the cement on to the centre of the convexity in the flint and replace the crown on it; press with the finger on the centre of the crown until the cement exudes all round from between the glasses; remove the lamp and support the plate of iron by a firm prop; then place a thick annular pad of filtering-paper, two-thirds of the diameter of the glass, on the crown glass, and on this a moderate weight, such as will press the glasses very close without being so heavy as to distort them; the weight had better be as warm as the glasses. Props should be arranged so as to keep the glasses *in situ*; and if they move on each other, a little adjustment may be made by the hands. Allow the whole to cool slowly; when cold, wash off the overflow of cement with naphtha or other solvent, and the operation is complete.

2nd. To filter rosin or similar substances when not quite clear.—Procure the most transparent rosin; I have obtained some at drysalters' in Long Acre, which in films of 0·1 to 0·2 of an inch has scarcely perceptible colour.

Make a single conical filter of white filtering-paper, suspend it by the ring of a retort-holder placed near the top, using no external funnel; put the rosin, roughly broken, into this filter, and place it a few inches in front of a bright parlour fire, turning it occasionally: the rosin will melt and drop from the filter beautifully clear. It may be collected in a vessel, or dropped at once into castor-oil or other liquid used; they should then be warmed and well stirred together, and may be put aside for use when required.

3rd. Levelling.—If the glasses thus prepared have not the meniscus of cement of uniform thickness for the same distances from the centre (a defect ascertainable in a moment by looking through the telescope at a real or artificial star), warm them again until the cement is melted, and no more, and with the two hands gently pinch the edges together, turning the glasses constantly round until cold, or nearly so. I have never found this fail, if neatly executed.

If air-bubbles appear in the cement, slide the one glass when hot over the other until the bubbles escape, and then gently slide it back.

Many details can only be learned by practice. I have given all the points as to materials and manipulation which occur to me as essential. I strongly urged on Mr. Cooke some years since to take up the subject, as I am, of course, not sufficiently a practical optician to make good telescopes. I hope it will now be taken up in earnest.

ON A MODE OF REVIVING DORMANT IMPRESSIONS ON THE RETINA.

Philosophical Magazine, May 4, 1852.

FIRST. Look steadily at a luminous object, sufficiently bright to be borne by the eyes without great inconvenience, then turn the eyes upon a dark body or dark space: an image of the object previously looked at will be seen, a fact familiar to everybody. When the image has completely faded away and is no longer visible, pass backwards and forwards between the eye and the dark body a white substance, say a sheet of paper: the image will be immediately revived, and may be thus indefinitely reproduced.

If the light is, in the first instance, not sufficiently vivid to produce the continued impression on the retina, but is nearly so, the invisible image may be brought out, or first rendered visible, by moving the white object between the eye and the dark body or dark space looked at. The white substance should be in a situation exposed to light, so that its whiteness affects the eye, and not held in shadow. After a little prac-

tice, it is astonishing to what an extent and for how long a time images may be thus reproduced.

2nd. Reverse the experiment, looking from the bright object at white paper, and a dark image of the object will be seen; when this has faded away, move between the eye and the paper a dark substance, held so as to reflect as little light as possible to the eye, and the image is reproduced on the white paper; or may be, in the first instance, produced, as with the converse experiment.

The explanation which occurs to me is, that the effect is one of contrast between the portions of the retina which have not been strongly affected and those which have.

The white paper dulls or deadens the sensitive portion of the retina for an instant, more than the part which has been previously rendered non-sensitive to other impressions than that which it has received by the bright light, and the black supervenes as a contrast to the parts affected by the white, but not to those unaffected. In the converse experiment the black relieves or renders more sensitive the comparatively unaffected portions of the retina, but has little or no operation on the non-sensitive parts; thus, at the moment of removing the black body, the unimpressed portions of the eye are affected by the white substance, but the impressed portion is comparatively dead to it.

PHOTOGRAPHIE NATURELLE.

Cosmos, Janvier 12, 1858.

Aux faits si intéressants et si curieux énumérés par M. Millot-Brûlé, vient se joindre une observation curieuse de M. Grove. Il s'agit cette fois de photographie naturelle des truites. M. Grove avait pris une grande truite, et n'ayant pas de baquet où il pût la déposer, il l'avait jetée au pied d'un arbre. Quand il revint une heure après, il s'aperçut qu'elle était couverte de larges taches blanches. En examinant avec grand soin, il remarqua que ces taches reproduisaient l'image de feuilles et d'herbes qui avaient été en contact avec la truite, ou placées

dans son voisinage. Cette remarque l'amena à faire l'expérience suivante. Il plaça de chaque côté de la truite deux feuilles dentelées sur leurs bords, et la déposa doucement sur le sol, de telle sorte que l'un des flancs, celui qui touchait la terre, fût tout à fait à l'abri de la lumière, tandis que l'autre flanc, au contraire, restait en pleine lumière. Au bout d'une heure, il enleva les feuilles, et vit sur le flanc supérieure une image nette et très-bien définie de la feuille, absolument comme si, dans les premiers ages de la photographie, on l'avait placée sur une feuille de papier sensibilisé. Sur l'autre flanc, au contraire, celui qui était à l'abri de la lumière, il n'y avait aucun changement produit. L'image de la feuille sur la peau de poisson était blanche ou blanchâtre sur un fond obscur. Il semblait que la lumière avait tout noirci à l'entour de la partie recouverte par la feuille. M. Grove avait conclu tout aussitôt à une action photogénique ; il lui reste cependant un doute qu'il n'as pas encore éclairci. La différence de couleur ne pourrait-elle pas être attribuée à un effet de sécheresse ou d'oxygénation ? Il ne le pense pas, parce que le côté inférieur était lui-même exposé à l'air, quoique moins librement, et que la feuille simplement placée sur le flanc supérieur la défendait assez mal du contact de l'air. M. Grove ajoute : 'Je crois avoir entendu dire que la truite change rapidement de nuance lorsqu'elle passe d'un ruisseau à l'autre, ou même d'une région à l'autre dans un même ruisseau. L'effet que j'ai constaté a de l'analogie avec la modification de teinte qui la lumiére solaire détermine sur la peau humaine ; mais chez la truite, en la supposant de même nature, elle est plus rapide. Les truites sur lesquelles j'ai expérimenté pesaient un kilogramme, elles étaient de la variété œillet-violacé.'

ON THE REFLEXION AND INFLEXION OF LIGHT BY INCANDESCENT SURFACES.

Phil. Mag., March 1859.

ON putting in order some old papers I found a manuscript in my own handwriting, and the subject of which I had en-

tirely forgotten; and it was not until some time had elapsed that I could recollect anything about the experiments contained in it. I now remember that they were made at the London Institution, and it must have been from ten to fifteen years ago. I have no recollection of the reason why I did not publish them, and can only guess that it was in accordance with my general habit of not publishing negative results. The results here, however, though negative, seem to me interesting, as positive results would *à priori* be expected; and may be worth publishing in the 'Philosophical Magazine.'

The difference in appearance to an observer of a polished surface when at ordinary temperatures and when ignited is sufficiently marked. The self-luminous character of the ignited body apparently removes the impressions of surrounding objects, and would lead to the belief that reflexion, at least that of the character yielded by polished surfaces, was destroyed. Such has been the *à priori* impression of those whose opinions I have asked on the subject. My own belief was, that if polished surfaces when ignited reflected light, they at all events broke up or scattered the reflected rays, and would cease to have the character of a polished surface; and that if they reflected light at all or notably, they would reflect it as paper or snow does, dispersing the rays so as to produce a general impression of luminosity, instead of throwing them back in a parallel beam, or one in which they preserved their original relative inclination.

The subject appeared worth investigation; and as I could not find that it had been attempted, I determined to make a few experiments upon it. The difficulty which immediately presented itself was, that the surfaces which are mainly employed for polished reflexion being oxidable metals, their physical structure would be changed by the oxidation consequent on incandescence. Gold or platinum, therefore, were the only substances which promised any success; and the latter, from its reflecting white light and more ready capability of retaining a high temperature, was selected. A strip of platinum-foil, 2 inches long by 0·2 broad, was firmly stretched on a piece of plate glass, polished with putty-powder and tripoli until it had reached as high a lustre as it could be made to

attain. One extremity was then fixed in a clamp attached to a wooden frame; and to the other extremity was attached, by a similar clamp, a metal weight, from which weight a wire extended and dipped into a vessel of mercury. The whole was arranged with care, so as not to bend or disturb the plane surface of the platinum. The foil thus suspended was brought opposite a vertical cleft in a window-shutter facing the meridian, which cleft could be made of any convenient size by horizontally moveable boards. The platinum-foil was placed opposite the cleft, so as to receive a sunbeam; a sheet of white paper was arranged directly in the path of the reflected beam; and the distance of the paper from the platinum was indefinitely varied during the experiment. Having accurately marked the boundaries of the reflected beam and its intensity, as far as the eye could judge, the platinum was made part of the circuit of a voltaic battery, the intensity of which was varied so as to produce effects on the foil varying from a heat scarcely visible in the dark to incandescence up to the point of fusion, or rather to the point at which the foil broke, from its diminished cohesion; for with a weight suspended, although not more than barely sufficient to keep the foil stretched, it always broke off at a temperature short of its point of fusion. In none of these variations, however, was there the slightest apparent difference in the reflected light on the paper. Or if, as occasionally happened, the shape a little changed during the progress of the experiment, it was fully explained by the elongation dependent upon the heat, or by the consequent removal of slight curvatures.

A similar experiment was made with diffused daylight, and with similar effects, also with the light from an argand lamp. In the latter case, when the reflected beam was so dim as to be interfered with by the light afforded by the incandescent platinum, the image was proportionately affected. Still it preserved its character, and, as far as could be judged, its intensity; and it was only by a very high degree of incandescence and a very feeble incident light that the reflected image seemed to merge in the direct light from the incandescent body.

I now brought my eye into the position where the paper

had been placed, so as to catch the reflected beam, while my assistant alternately made and broke contact with the battery. When the incident sunlight was sufficiently intense to mask the emitted light of incandescence I could not, in the slightest degree, distinguish whether the platinum was ignited or cold. When I first tried it I two or three times complained wrongly to my assistant that he had not made contact when I told him to do so; when the incident light was very dim, the emitted light was, of course, also distinguishable. I now caused the spectrum from a flint-glass prism to fall on the platinum, and with similar effect; *i.e.* when the reflected spectrum was very intense, no difference could be detected between the light from the platinum, whether cold or ignited, or whether received upon paper or upon the eye; when less intense, the red portion of the spectrum was elongated by the light of incandescence, and the other portions partook of the character of the spectrum superposed upon, or blended with, the light of incandescence.

The prism was also arranged so as to intercept the reflected instead of the incident beam; the effects were similar.

A beam of light polarised by reflection at the proper angle from a plate of glass was made to fall on the platinum surface, and then analysed by a tourmaline; no difference was perceptible in the plane of polarisation, whether the platinum was ignited or not.

The light reflected from the platinum was similarly polarised and analysed, but no difference dependent upon incandescence was detected.

A wire of platinum, 6 inches long and $\frac{1}{30}$th of an inch in diameter, was vertically suspended in the narrow fissure of the shutter; the bands of interference were received on paper placed at different distances from the wire, and examined both by the eye and by a lens; no difference could be detected in these bands when the wire was ignited by a voltaic battery.

In all the above experiments the foil or wire was ignited by the battery previously to the commencement of each class of experiment, so as to avoid any effect arising from the alterations caused by the platinum having been subjected to heat,

such, for instance, as the effect of annealing might produce, or the burning off from it of films of moisture, or of oxidable substances.

The general result of these experiments is, that no difference is perceptible by the eye in light reflected by a polished surface, whether that surface is ignited or not; that the superficial molecular uniformity, which causes a bundle of parallel rays to preserve their parallelism of direction when reflected, is, if the ignited substance be inoxidable, not broken up by ignition.

I know of no photometer which would be suitable for indicating their effects with greater accuracy than the eye; but, although these results lead to a conclusion different, I believe, from that which would have been arrived at *à priori*, they by no means exclude the possibility or even probability of some difference being produced in the direction or character of light reflected from ignited surfaces as compared with that reflected from unignited surfaces.

The fixed lines in the spectrum, for instance, differ materially according to the source of light; and even supposing the ignited surface to make no difference in the character or position of the fixed lines of reflected solar light, a point which I have no apparatus sufficiently delicate to detect, yet there is every probability of novel and valuable results being attained by the interference of this light of incandescence with that of solar or other light reflected from the incandescent body, the same body being then, in some sense, the source of two different descriptions of light, which differences are capable of detection by the different position and character of the fixed lines in their respective spectra. Such experiments, and many others which they obviously suggest, appear to me to offer an interesting field of experiments in physical optics; and to those who are more practically acquainted with this science than I can pretend to be, and who may possess more delicate means of detecting minute effects, I therefore leave them.

NOTE ON THE OCCULTATION OF JUPITER, NOVEMBER 8, 1856.

Proc. Astronomical Society, Nov. 14, 1856.

I HAD a good position for witnessing the partial occultation of *Jupiter* from an upper room of my house, situate opposite Clarence Gate, Regent's Park. I used a small but unexceptionably good achromatic telescope, $1\frac{3}{4}$-inch aperture and 22 inches focus; the magnifying power was 43.

It seemed to me—(but I attach little value to the observation, as the effect might have been due to an agitation in the earth's atmosphere)—that at the moment before immersion the margin of *Jupiter* was slightly elongated, or, so to speak, stretched itself out to meet the moon; this was followed immediately by a flattening of the same portion of the disk.

Throughout the whole period a dark line was plainly perceptible between *Jupiter* and the moon. This line appeared to me straight, as though cutting off the convexity both of *Jupiter* and the moon. The light of *Jupiter* was notably less brilliant than that of the moon; I should say hardly equal to half the intensity of the latter. It was of a much bluer tint than the moon, probably an effect of contrast arising from the different intensities of their respective lights. The belts of *Jupiter* were plainly seen throughout the occultation, and, as far as I could observe, were not in any respect distorted.

The dark line I thought at first arose from the intervention of a fine rim of the dark body of the moon, but this could hardly be the case towards the termination of the occultation. About a fifth part of the planet appeared to me to be occulted at the maximum period.

NOTE ON THE OCCULTATION OF JUPITER OF JANUARY 2, 1857.

Proc. Astronomical Society, Jan. 4, 1857.

OWING to the kindness of Mr. Gillett, I was enabled to observe this occultation through his binocular telescope. This

instrument is by Ross, the object-glasses $3\frac{3}{8}$ inches diameter, focal length 51 inches.; portions (about $\frac{3}{4}$ inch) of the glasses are ground away to enable them to be approximated sufficiently for the distance between the eyes of the observer. The power used was 60, which gave a greater idea of magnitude than double that power in a monocular telescope.

The dark limb of the moon was faintly visible throughout the time of the occultation. The disappearance of the satellites was not instantaneous, as with a fixed star, but (as indeed might have been expected) they gradually faded away. The light of the moon's edge was too feeble for me to ascertain whether they disappeared at the exact edge or not. I should say that they seemed to be a little within it, but I could not positively assert this. *Jupiter* was cut off with perfect sharpness, and I could not detect the slightest distortion. When about one-third was eclipsed the light of the planet appeared brighter along the edge of the moon, and this apparently increased brightness continued until it was about three-fourths eclipsed. Two spots in the neighbourhood of the upper belt (as viewed by inverting telescope) were notably bright, so as to attract attention, and they continued so during the period above-mentioned, each looking somewhat like an electric glow coming off the dark edge of the moon. This effect may be optical, and caused by the immediate proximity of darkness to central and more luminous portions of the planet, but it was very marked. I observed no other effect during the period of immersion. I looked for an effect said to have been observed on previous occasions, viz. a faint illumination of the dark edge of the moon upon contact, but could not detect the slightest trace of it. On emersion I could detect no distortion of any sort, nor did I see the dark line which I noticed in the occultation of Nov. 8, 1856. The difference of light and colour between the planet and the moon was the same as then observed.

After emersion there was a decided stereoscopic effect, seen both by Mr. Gillett (who up to this time had left the telescope entirely to me) and myself; *Jupiter* in the binocular appearing as at a distance behind the moon. Mr. Gillett tells me that with terrestrial objects the effect of the binocular, if

inverting eye-glasses are used, is in many instances pseudoscopic, the more distant objects appearing nearer than the more proximate, which is the effect that theory would indicate.

The satellite, last in order on emersion, seemed to cling for a second or so to the moon's edge, probably an effect due to irradiation. I did not observe this with the three others, but I did not catch the moments of emersion so well with them. I was greatly struck by the beautiful impression conveyed to the mind by the binocular telescope; and judging from what I had seen with a monocular telescope of my own, of 4·3 inches aperture, and therefore not far differing in area from the compound area of the two object-glasses in the binocular, the latter was decidedly superior, and the ease and, if I may be allowed the expression, the *naturalness* of the vision, was very remarkable.

NOTE ON COMET V. 1858.

Proc. Astronomical Society, Nov. 12, 1858.

THE following extract from a letter adressed by Mr. Grove to Mr. De La Rue refers to the phenomena observed during the transit of the tail of the comet over *Arcturus* on the evening of October 5. The telescope employed by Mr. Grove was a small instrument, of only two inches aperture, and his remarks are offered under an impression that the weather elsewhere was not generally favourable for observing the comet on the interesting occasion to which they refer:—

'When the comet had entered well within the margin of the tail a dark notch was formed, cutting out a portion of the tail round the star; and as the star got farther in, this became a dark areola surrounding the star, and in diameter equal to about one-tenth of the line of transit. This continued until the star reached the middle; at this part there is a broad dark

line which extends from the nucleus to a distance considerably above the point where the star crossed. When *Arcturus* arrived here this dark space was perfect up to the star, but on the other side the white light of the tail appeared to come quite up to the star; in short, the bright part of the tail was darkened in the vicinity of the star, and the dark part was brightened, at least so much of it as was on the side farthest from the nucleus.

'I saw the notch again on the opposite side previous to emersion, and then lost it by clouds.

'The effects I have described are, doubtless, optical, and the notch and areola evidently due to the bright light of this star; the effect on the dark central part is not so easy to explain.'

NOTE ON OCCULTATION OF SATURN, 1859.

Proc. Astronomical Society, May 8, 1859.

I OBSERVED the occultation of *Saturn* with a six-foot telescope, 4·3 inches aperture, power 165. I imagined that upon the moment of first contact the ring was a little drawn out into a point, succeeded by a knot or apparent excrescence; but on thinking it over since it has occurred to me that, as I could not see the edge of the moon's dark limb, and had no clock or means to advise me of the moment of impact, the effects I noticed might well have been due to the cutting off, or, as it were, sharpening the point of the ring by the oblique incidence. I mention the apparent effect, however, as if better observers have noticed it there may be something in it as a corroboration. Excepting this, from the impact until the total disappearance, I could not notice the slightest distortion.

On the emergence the extremely faint light of *Saturn*, contrasted with the moon, was very remarkable; *Saturn* was a mere ghost of himself, a faint grey-blue shadow. The line of the moon's edge was perfect, and no dark band was percep-

tible, as I and others noticed in the occultation of *Jupiter*. The part of the ring last emergent appeared flattened or shorter than the opposite portion, doubtless from the greater contrast of light or from irradiation. I think the former was the cause, as some disproportion continued after *Saturn* was clear of the moon, and distant from the edge about the diameter of its (*Saturn's*) own orb. The atmosphere was better than the average for the early time of the night, and I doubt not some good observations will have been made.

NOTE ON APPEARANCES OF MARS, 1862.

Proc. Astronomical Society, Dec. 12, 1862.

MR. GROVE, in a letter dated November 5, 1862, gives an account (illustrated by three pencil sketches) of the appearances presented by the planet *Mars* on October 26 and 31, and November 3. The telescope used was $4\frac{1}{2}$ inches aperture; focal length, 6 feet 2 inches; object-glass by Cooke, of York, capable of dividing η *Coronæ*. He is perfectly satisfied of there being notable changes in the distribution of the lights and shadows, inconsistent, it appears to him, with their being due to land and water, or, as he should perhaps say, land and water *only*; clouds condensed over large aqueous districts might possibly account for the changes observed; the changes of position by axial revolution would not explain them.*

THE AURORA BOREALIS OF 1870.

From 'Nature,' Nov. 1870.

I SHALL be obliged if you will put on record a few scattered notes which I took of the splendid Aurora Borealis of Oct. 25, seen from Arthingworth, Northamptonshire. When I first observed it, at half-past five P.M., a crimson glow extended in

* This notice requires the sketches, and I cannot find them.—W.R.G., 1874.

an irregular band from NNE. to W., most prominent at about 20° to 30° above the horizon. This increased in height and breadth until it nearly reached a point SW. of the zenith, and about 15° WNW. of the star *Vega*. At this time the northern part of the sky was perfectly free from aurora; gradually that part and the whole dome of the heavens, with the exception of a section from W. to nearly S., became filled with luminous streamers. These, for about 20° on each side of N., were white, the others crimson striped with white or rather greenish light; but the green I believe to be an effect of contrast, as where similar streamers were distant from the red light they were white.

The white or green streamers appeared to eclipse the red light; they changed their size, shape, and position, while the red continued comparatively unchanged. There were also dark streamers, which might have been mere spaces without light, and be caused by the darkness beyond, but they certainly appeared to form a part of the phenomenon itself. These streamers or long brushes could be seen beyond and clear of the luminous portion of the aurora, leaving the normal light of the sky between them and it, and hanging like long horse-tails, or like the fringes of rain seen on the edges of a distant rain-cloud, changing their shape and position just as the luminous streamers are seen to do.

The most remarkable part of the phenomenon, however, was the circle of sky, or what may be called the pole of the aurora, to which the streamers converged. It appeared to embrace about from 7° to 10° of space. To an ordinary observer it might have seemed occasionally to shift its position to some extent, but, as far as I could judge during an hour's observations, this was not really the case; flickerings at times covered portions of it, and at other times the whole became faintly luminous; but by marking its position with reference to some small stars the position of the circle seemed to me to be unaltered. Most singular were the terminations of the streamers as they culminated at this circle, not being undefined or gradually evanescent, but having angular tips far brighter than the portions immediately beneath, the nearest illustration to which I can give is an inverted fish-tail or bat's-wing gas-

burner, except that this gives a feeble light at the point, while the aurora tips were whitest and brightest there, the streamers now fading off, and now becoming brighter and tinged with red as they got to 40° or 50° from the horizon ; the tips varied constantly, but preserved the mean distance from the pole or focus of the aurora. The position of this was, as far as I could ascertain without star-maps or instruments for observation, about 15° WNW. of *Vega*. The convergence of the beams was not in appearance conical, but dome or cupola shaped ; this was, however, in all probability an optical illusion. Whether there was really a convergence or whether the beams were parallel, and the convergence an effect of perspective, can only be decided if some approximate measures of the distance of the streamers be ascertained. It appears to have been at a greater distance from the earth than is usually attributed to aurora borealis, having been seen in different parts of Europe and, I believe, in America. Doubtless the comparison of these observations will give some parallax or approximation to measurement of the distance. I remember about seven or eight years ago seeing an aurora at Chester, where the flashes appeared close to the observer, so that gleams of light continuous with the streamers could be seen between the houses of the town and myself, like the portions of a rainbow intervening between terrestrial objects and the observer. I tried then to ascertain if there was any reflection or other cause of optical illusion, but could not see it as other than a real effect ; I seemed, so to speak, to be in the aurora. The effect on the 25th was very different, and gave me the idea of great distance.

The light was sufficient to enable me to tell the time by my watch easily, but not to read newspaper print.

Between half-past six and seven o'clock it faded away, and at from half-past seven to ten had become an ordinary white aurora, confined to the northern portion of the heavens.

ARTIFICIAL ROCKING-STONES—AN EXPERIMENT.

Communicated to the Geological Sec. of the British Association, Norwich. Norfolk News, Aug. 25, 1868.

SOME short time ago, during an excursion in Cornwall, my attention was naturally directed to rocking-stones, and those approximations to rocking-stones which are seen in the granite where it is exposed to the action of the heat and cold, air and water. I presume that I need not argue here that rocking-stones are natural results, and not superposed on their pedestals, as was once believed, by the hand of man.

Throughout the greater part of the granite rocks of the west coast of Cornwall formations are to be seen approaching in character to rocking-stones, or to discoid piles, like the Cheesewring.

If we suppose a slab of stone of a parallelopiped form lying on another, both having flat surfaces, or in other words such slabs as are formed by fissures in horizontal and perpendicular directions which are common in exposed granite rocks, the attrition and disintegration produced by changes of weather, of temperature, &c., would necessarily act to the greatest extent at the corners and next to that at the edges, because those parts expose respectively the greater surfaces compared with the bulk of the stone. This would tend to round off all the angles, and gradually change the rhomb more or less towards an oblate spheroid. This would account for the Cheesewring, &c. But then, it may be asked, why should this process gradually work on to a rocking-stone? in other words, why should the last unworn point, points, or line be in the line joining the centre of gravity of the upper stone with that of the earth? Such an accident, it may be said, might happen, but the chances are almost infinity to a unit against it. Not so. Assume the wearing away between the slabs to reach a point which is not in the line of centres of gravity, the upper stone would then fall on one side, leaving the unworn point more exposed to climatal and probably to electro-chemical action from the water lying in the angle of the crevice, evaporation

being less rapid there than at other parts. This point would then be worn away, and the stone would fall back a little; then fresh action upon new surfaces, another oscillation, and so on. The effects which I have explained as taking place by steps would, in fact, take place by insensible progression. By assuming this process, unless there be some interfering action, it becomes not improbable that the last point or line worn away would be the point or line on which, from its being in the line of centres of gravity, the upper stone would rock. After seeing the great Logan-stone, near the Land's End, I traced so many other approximations to rocking-stones along that coast that I became satisfied, as far as one ought to be satisfied on any subject of human enquiry, that this was a correct theory. It then occurred to me, if this view be true, may we not be able to hasten the operations of nature so as to produce artificially (if such a term may be used) the rocking-stone results? A very little thought suggested the experiment. Two parallelopipeds of iron, which had been made for keepers of magnets, were taken, similar each to each, but that one was twice the length of the other. The shorter was superposed on the longer, and both immersed in sulphuric acid diluted with three times its volume of water; some nitric acid was added at first, to hasten the corrosion. The liquid was changed from time to time as it became nearly saturated, but without changing the position of the iron. At the end of three or four days the pieces of iron were taken out, washed, and examined, when the upper one was found to be a perfect analogue of a rocking-stone, so delicately balanced on two points that it could be made to rock by blowing on it with the mouth. (Result shown.) It will be observed in this experiment that the iron rocks only in one direction. Such is the case with the great Logan-stone, and I believe with the greater number of rocking-stones. It is obviously more probable that a stable equilibrium should be obtained on two points than on one. I have not yet got a specimen to rock or spin upon one point. (Approximation to this shown by two zinc discs and explained.) If the surfaces of the slabs be in such close contact that there is not room for circulation of the saturated liquid, a formation like those near the Cheesewring will be effected; or if a num-

ber of discs or slabs be superposed, and the lower ones more exposed to the weather, and to catch the dripping and drifting water from the upper, we should get a formation exactly like the Cheesewring, which may be called an incipient compound rocking-stone, in that each slab is worn away at the edges, and the lower ones much more than the upper, so that if left alone, which it won't be, and if it does not topple over too soon, which it probably will, it might well end in a rocking-stone. I should not be surprised if it rocked now in a great storm.

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